PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. IV. 1859–60. No. 52.

Monday, 16th January 1860.

The following Gentlemen were duly elected Ordinary Fellows:—

William Robertson, M.D., F.R.C.P.E.
Frederick Guthrie, B.A., Ph. D.
James A. Wanklyn, M.R.C.S. Lond.

Monday, 6th February 1860.

Sir DAVID BREWSTER, Vice-President, in the Chair.

The death of General Sir Thomas Makdougall Brisbane, Bart., G.C.B., President of the Society, was announced, as having taken place at Brisbane on 27th January; thereupon the following motion was made by Lord Neaves, one of the Vice-Presidents of the Society, and unanimously agreed to:—

"That the Society, at this its first meeting after the death of Sir Thomas Makdougall Brisbane, should place upon record the expression of its deep regret for that event, and its high estimate of the character of Sir Thomas Brisbane, who, besides other eminent public services, was during a long life conspicuous as a sincere lover and active promoter of science, and who so worthily presided over this Society for a period of twenty-seven years.

"That an excerpt from this minute be sent to Lady Makdougall Brisbane, with an expression of the Society's sympathy and condolence."

The following Communications were read:—

1. On the Capture of Whales with the aid of Poison.
By Dr Christison.

This paper was an account of a plan proposed and tried many years ago for poisoning whales, by introducing hydrocyanic acid into their bodies when struck with the harpoon.

In 1831, the author was consulted on the subject by the late Messrs G. and W. Young of Leith, at that time concerned in the northern whale-fishery. It appeared to the author that the idea was feasible; that it was sufficient to paralyse the animal, so that the boats could speedily come up with it; and that a quantity of concentrated hydrocyanic acid, adequate to produce that effect at least, could be introduced by means of two glass tubes, one on each side of the shank of the harpoon, where they would be protected by the barbs of the blade. After various devices were thought of for breaking the tubes at the right moment, the contrivance fixed on was, that the double barb, with which each side of the blade terminates, should be jointed movably on the upper portion of the blade; so that, when traction was exerted, on the whale starting off with the harpoon in its body, the external ends of the barbs would be thrown outwards so as to increase the hold of the harpoon, while the inner ends of the barbs would be squeezed against the tubes, and crush them to pieces.

Harpoons so constructed, one of which was exhibited to the Society, were sent out in a Greenland ship, with a large supply of concentrated hydrocyanic acid, in the spring of 1832; but the ship was nipped between two ice-fields on its first arrival among the whales, and just when all was ready for the trial. In 1834, another vessel was sent out by the Messrs Young, provided with the same kind of harpoons, and with a harpoon gun for firing them. Not having been aware till lately of this second trial, the author had been able to inquire into the result only a few weeks ago. The accounts he received were discrepant. According to the most credible report, the experiment succeeded; but the crew were so alarmed at the effects, that they were afraid of being poisoned in removing the blubber, and would not persevere in using the harpoons.*

The paper concluded with observations, showing that increased facilities might be secured, in consequence of the comparative ease with which concentrated prussic acid might now be made and preserved in hermetically sealed tubes, and likewise because poisons were now known even more potent than hydrocyanic acid.

2. Notice regarding the Branchial Sac of the Simple Ascidiæ. By Andrew Murray.

In a paper which I read last year before this Society, "On the Structure and Functions of the Branchial Sac of the Simple Ascidiæ," I stated that I had fed and injected ascidiæ with indigo and other coloured sea-water, and that in those so fed, the coloured material was never found on the exterior of the sac, but always deposited on the inner wall, and that injection by the mouth into the sac failed to push the coloured matter through its walls, except by rupturing them.

These experiments were not isolated or few in number. I showed the results to various friends—among others, Professor Goodsir, Dr Wright, and Dr Cleland—and never, in any of the experiments which I made, did a single instance occur of the indigo passing through the stigmata of the sac; and, in that paper, I naturally reasoned from this fact. Subsequent observation has shown me that, in relying upon these negative instances, I was generalising too hastily. Since I read that paper, I have in two instances found, on feeding the *Ascidia virginea* with indigo, that it had passed through the windows of the sac and was partially deposited on each side of the sac, and some of it sticking in the meshes; and I hasten to correct the erroneous impression conveyed by the negative instances in my former paper. Of course, all the reasoning in it, founded on the supposed impenetrability of the sac, falls to the ground; and it would appear that my error has arisen from the animal having the power of rejecting or passing through these stigmata the contents of the sac as it pleases, which accounts for various other observers having seen currents running past the fenestræ without entering them; and my previous experiments probably failed from the fact of the indigo being distasteful to the animal, which exercised its power of not admitting it to pass the walls of the sac.

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* Since the Author's paper was read, he has received distinct information, that at the first trial, the whale "sounded," but in a surprisingly short time came up again—dead.

Another correction which I wish to make is this: I stated that Dr Wright and I thought that, under a high power of the microscope, we saw a diaphanous membrane stretching across the branchial stigmata, which showed a polygonal structure similar to that of epithelica. I have never been able to detect this again; but I have seen something approaching to it, which I am satisfied was a compressed aggregation of blood-globules, and I strongly suspect (particularly since finding that the indigo has passed through these stigmata) that my former observation is to be referred to some deceptive appearance of this kind.

The following Gentleman was elected an Ordinary Fellow —

Professor MACDOUGALL.

The following Donations to the Library were announced:—

On Hypsometrical Measurements by means of the Barometer and the Boiling-Point Thermometer. By J. Burgess, F.E.I.S., Calcutta, 1859. 8vo.—*From the Author.*

Reminiscences of General Sir Thomas Makdougall Brisbane, of Brisbane and Makerstoun, Bart. Printed for private circulation. 4to. 1860.

Memorial on the new system of Dioptric Lights, invented and introduced by Sir David Brewster, K.H., to the Lords Commissioners of Her Majesty's Treasury. 8vo. 1859.—*From the Author.*

On the Life-Boat, the Lightning-Conductor, and the Light-House. By Sir David Brewster, K.H.—*From the Author.*

Transactions of the Royal Scottish Society of Arts. Vol. V., Part 3. 8vo. 1860.—*From the Society.*

Schriften der Universität zu Kiel, aus dem Jahre, 1858. 4to.—*From the University.*

Charts and Sailing Directions.—*From the Dépôt de la Marine.*

Monday, 20*th February* 1860.

DR CHRISTISON, Vice-President, in the Chair.

The following Communications were read :—

1. On the Action of Uncrystallised Films upon Common and Polarised Light. By Sir David Brewster, K.H., F.R.S.

Since the discovery of the polarisation of light by refraction, the action of a pile of transparent plates upon common and polarised light has not been studied by any of the writers on physical optics. It was believed that a pencil of common light was completely polarised in the plane of refraction when the plates were sufficiently numerous, no special notice having been taken of the light thrown back by reflexion into the transmitted and polarised beam. Sir John Herschel, indeed, had referred to it; but he remarks that "it mixes with the transmitted beam, and, being in an opposite plane, destroys a part of its polarisation."* So long ago as 1814, Sir David Brewster had shown that this reflected light is distinctly visible as light polarised by reflexion;† but owing to the difficulty of procuring very thin plates of glass with perfectly parallel surfaces, it was impossible to ascertain the true character of the oppositely polarised pencils.

Having obtained, however, films of decomposed glass of great thinness, and perfectly colourless, the author was enabled to prove that the transmitted beam consisted of two pencils oppositely polarised, and that when polarised light was incident obliquely on such a pile, and subsequently analysed, the pile of films exhibited all the properties of a plate cut perpendicular to the axis of a negative uniaxal crystal, the tints produced by the interference of the pencils rising to the *blue* of the second order of Newton's scale of colours, by increasing the obliquity of the incident pencil.

A line perpendicular to the plates or films at the point of incidence corresponds with the axis of the uniaxal crystal; and the different azimuths in which the polarised ray may be inclined to this axis correspond with the principal sections of the crystal.

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* Treatise on Light, Art. 868.

† Phil. Trans., 1814, p. 226.

When the films of decomposed glass are circular spherical segments and colourless, the black cross and its accompanying tints are finely displayed, as in the system of rings seen along the axis of uniaxal crystals. When the films have the colour of thin plates, and are deeply spherical segments, the tints of the rings which accompany the black cross are singularly modified.

2. On Mr Darwin's Theory of the Origin of Species. By Andrew Murray.

The position taken by Mr Darwin is, that all species have arisen by the natural process of ordinary generation. That the differences which we now see in them have arisen from slight variations in individuals having from time to time occurred, which have been perpetuated by inheritance, by successive stages and slow degrees, through unlimited spaces of time. Some of these slight variations he considers to originate in causes beyond our power of explanation. and which, although not the work of chance, we may call chance, for want of a better appellation—others to arise from habit, or from the excessive use or disuse of certain organs; but that when such a variation has once appeared, it is preserved by hereditary descent through a principle which he calls "natural selection," and which he deduces as a corollary from the struggle for existence which we see constantly going on around us. "As many more individuals," says he, "of each species are born than can possibly survive; and as, consequently, there is a frequently recurring struggle for existence, it follows that any being, if it vary, however slightly in any manner profitable to itself, under the complex and sometimes varying conditions of life, will have a better chance of surviving, and thus be *naturally selected*. From the strong principle of inheritance any selected variety will tend to propagate its new and modified form" (p. 5). Mr Darwin by no means shuns pushing his theory to its legitimate conclusion. In arguing as to the acquisition of new habits by some of his supposed transitional animals, he says, "In North America the black bear was seen by Hearne swimming for hours with widely-open mouth, thus catching, like a whale, insects in the water. Even in so extreme a case as this, if the supply of insects were constant, and if better adapted competitors did not already exist in the country, I can see no difficulty in a race of bears being rendered by natural selection more and more aquatic in their structure and habits, with large and larger mouths, till a creature was produced as monstrous as a whale" (p. 184).* And the final conclusion to which he has arrived is summed up as follows:— "Analogy would lead me one step farther, namely, to the belief that all animals and plants have descended from some one prototype. But analogy may be a deceitful guide. Nevertheless, all living things have much in common in their chemical composition, their germinal vesicles, their cellular structure, and their laws of growth and reproduction. Therefore, I should infer from analogy, that probably all the organic beings which have ever lived on this earth have descended from some one primordial form into which life was first breathed" (p. 484).

Such is a general statement of the position taken by Mr Darwin; and in support of it, as might be anticipated from so accomplished a naturalist, we have in his work not only the chief arguments on which it rests ably stated, but numerous phenomena and facts in natural history applied to it, so as to test its probability by its consistency or inconsistency with them. These illustrations form, however, only a very small portion of the facts which he has accumulated, and which, he informs us, will be afterwards published in a larger and more elaborate treatise, and are now to be looked upon as no more than mere indications of the nature of the evidence he possesses, and proposes hereafter to adduce. A few of the most important of these I shall briefly notice, but I think their value may be perhaps better appreciated if I first state what I consider to be the essential qualities requisite for the existence and preservation of a species. I conceive that all species bear implanted within them two essential laws, without which they could not exist. The one, a power of accommodating themselves to a certain extent to circumstances; in other words, a power of modification or variation, as Darwin calls it. Without this the individuals composing the species would, under any change of circumstances, die, and, of course, the species would die with them. Now, it is not difficult to prove that this power of modification is possessed by plants and animals. I may instance the change which takes place in the wool of sheep, according as the animal is transferred from one climate to another—the change in the size of the chest and lungs which is said to take place in the second generation of animals transported from ordinary elevations to the intensely rarified air of lofty mountains, or the alteration that is found in shells, whether fresh-water or marine, when transferred into brackish water. But for evidence of this I need not go beyond the examples given by Mr Darwin himself. I think that all the instances of variation mentioned by him may be referred to this principle of modification. To this principle, and as designed for a similar purpose, do I refer the phenomena of hybridization. Putting aside a few exceptional cases, which may be explained on special grounds, I conceive that the well-known and undeniable general fact, that two distinct species may produce hybrid offspring, which hybrid offspring will be sterile either in the first or second generation, is strictly an instance of modification, allowed and intended for the preservation of the species. Conceive, for instance, a herd of deer, or any other animal, of which all the males have died off—conceive it to be the last herd of that species on the face of the earth. Except for this power of hybridization, the species is extinct, although it yet lives. Its propagation is at an end. No young can replenish its numbers, and the species endures only until the last individual has died off. But with the power of having fertile intercourse with a distinct species, another chance is given for its preservation. A hybrid is born; and if a male, it can have fertile offspring from the females of the original herd, and in a few generations all trace of the foreign blood will have been washed out. Such, I conceive, to be the uses of the principle of modification in its various phases, viz., the preservation of the species by the preservation and propagation of the individual. But the species may be lost in another way than by the death of its component individuals. It might, were there no check upon this power of modification, be lost by hybrids and modified individuals taking the place of species; in fact, were the power of variation unlimited and uncontrolled, all species would be confounded, and there would be nothing but an indiscriminate mass of creatures running all into each other, as should be the case under Mr Darwin's theory were it true in fact.* Centaurs and mermaids, nay, even dryads, would cease to be impossible fables, and the beauty of creation would be lost in one undistinguishable chaos. To guard against this, and to preserve species from extinction by confusion, as well from extinction by death, nature has furnished species with another attribute as a counterpoise to the facility of modification and variation, and that is the tendency to reversion to type. This is seen working in two ways; the one in the reappearance of typical forms or peculiarities after having been absent for one or more generations. We see it well in our own race, where a parent's face and talents, lost in the child, reappear in the grandchild—where even hereditary diseases show themselves after the intermission of a generation or two. This phase of reversion to type is slightly alluded to, and slightingly admitted as an element by Darwin. But the second, and, as it appears to me, by much the most important phase of reversion to type (and which is practically, if not altogether ignored by Mr Darwin), is the instinctive inclination which induces individuals of the same species by preference to intercross with those possessing the qualities which they themselves want, so as to preserve the purity or equilibrium of the breed. I again refer to our own race for an apt example. It is trite to a proverb, that tall men marry little women, tall women little men; a man of genius marries a fool, a great beauty the ugliest man she can find; and we are told that this is the result of the charm of contrast, or of qualities admired in others because we ourselves do not possess them. I do not so explain it. I imagine it is the effort of nature to preserve the typical medium of the race. Did a different feeling prevail, we should have our species broken up into giants and dwarfs, Newtons and idiots, Venuses or Apollos and satyrs, Sampsons and weaklings; or, if we should adopt Darwin's notions, the dwarfs, weaklings, and idiots, would all be extirpated by the predominancy of the stronger varieties. Now we know that this is not the case;

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* In quoting this, I do not at all mean to give it as a fair illustration of Mr Darwin's views. I only refer to it as indicating the extent to which he is prepared to go. The example here given I look upon (as I have reason to know Mr Darwin does himself) merely as an extreme and somewhat extravagant illustration, imagined expressly to show in a forcible way how "natural selection" would operate in making a mouth bigger and bigger, because more advantageous.

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* One of Mr Darwin's explanations of the absence of intermediate forms may be taken as his answer to this objection—viz., that these forms are, in point of fact, numerically weaker than the forms on each side which they link together, and thus are liable to be exterminated sooner than them. But, admitting the fact to be that they are less numerous, why should they be so under Mr Darwin's theory? With unlimited powers of modification, why should the intermediate forms always be *originally* fewer.

and we may guess how strong the instinctive inclination for reversion to typical form is, when we look abroad among our acquaintances and see, notwithstanding the manifold inducements to disregard the promptings of nature consequent upon the artificial state in which we live, how few have refused compliance to this mysterious law. The control of parents, the desire for easily acquired wealth, the promptings of ambition, the cravings of vanity, and the accidents of opportunity, all suggesting other matrimonial connections, and, backed with what may be looked on as of more importance than either, the strong control over one's own feelings and desires acquired by the habits of civilised life, generally give way before this imperious constraint for reversion to type. It is less easy to give similar evidence of this phase of the revertive principle in other animals. In the wild, we only see its result in the uniformity of all individuals; in the domestic, man interferes, and by his breeding compels departure from the type, and increases it. But I believe it requires man's greatest care and watchfulness to prevent reversion, and that a breed neglected retrogrades in a very short time; and what is called the prepotent influence of pollen from the typical plant over that of neighbouring varieties is an instance which will be admitted by most hybridizers; and an analogous influence may be equally exercised in the case of hermaphrodites and fixed animals. This is my belief: but it is not that of all; the *possibility* of the new variety made by breeders and gardeners reverting to their parent forms is doubted by many, and denied by some. Mr Darwin of course disputes it, or at least does not admit it, and desiderates the evidence on which the statement has been so often made, that our domestic varieties, when run wild, gradually but certainly revert in character to their aboriginal stocks. Such a demand for proof may not be capable of immediate satisfaction. But where a fact is very generally accepted* as true, it will usually be found to be based on some foundation. *Vox populi, vox Dei*, is true in more sciences than politics. Passing this, however, I would next notice that the phenomena of hybridization do not stop with the law allowing the hybrid to have fertile offspring from the parent stock; there is another law which prevents it having such offspring from other hybrids or other species, and this is quite in accordance with my view of the precautions adopted by nature for the preservation of species. In the first place, *fertility*, to preserve it from extinction by extirpation of individuals; in the second place, *sterility*, to preserve it from extinction by confusion of races. Such are my views of the purpose and working of the compensating qualities implanted in species. And my first objection to the principles on which Mr Darwin's theory rests is, that it is founded on exaggerated and undue estimate of the one—the power of modification; and if not a negation, at least an inadequate concession of the other, viz. the principle of reversion to type.

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* The point is one well worthy the attention of those who may have the opportunity of testing it. I have no doubt that many unscientific breeders could give at once instances which would bear upon it; but it will be observed that the question of whether they do bear upon it is one not unattended with difficulty: for instance, in our breeds of cattle how are we to know when a race or variety is reverting to its parent type—what was the parent form of our domestic cattle?—*Quien sabe.* But that they naturally retrograde or go away from the something which has been the aim set up in breeding to something else, certainly cannot be denied.

Seeing, then, that the power of modification or variation is the principle on which his whole superstructure rests, Mr Darwin wisely takes care to fortify it by adducing striking instances illustrative of the extent to which this may take place. As the power of modification is to be seen in its most developed form in domestic animals, it is from them chiefly—indeed, so far as support to his theory goes, I may say it is from them entirely—that his illustrations are drawn. Now, it is usually said that domestic animals are not fair examples from which to reason in inquiries into species and their origin; and it is thought that the artificial circumstances under which they live alter their system so much as to render any argument drawn from them not worthy of reliance. I have no doubt that such artificial life and great change of habits has an important effect upon these animals, and more especially upon their reproductive system, different conditions of which (as Mr Darwin has well shown) have much effect in inducing subsequent variations in their descendants; but, as already said, I imagine a still more potent cause of the greater variability of domesticated animals to lie in their being deprived, through the agency of man, of the opportunity of allowing the revertive principle to come into operation by intercrossing. But it is no part of my argument to dispute the power of variation within certain limits; and as, for the above reasons, I admit them to be greatest in domesticated species, I need not dally over the instances given by Mr Darwin, however interesting and suggestive they may be. I shall merely observe, with regard to those facts which he adduces as instances of natural variation, in contradistinction to artificial or domesticated variation, that they are limited, and consist (as it appears to me) entirely of such minor modifications as I have already alluded to, as permitted more or less in all species for the preservation of the individual. Seizing upon the variations (of which there is no want) which have given rise to disputes among naturalists as to whether particular forms are mere varieties or true species, he thence infers that these are species in the course of transmutation. That some naturalists, with too quick a perception of differences, should attempt to make species or sub-species out of varieties, should not prejudice the question; the blunders of the few are frail ground on which to rest a theory; and if the concurrent opinion of the majority be taken, the number of forms as to which doubt may fairly be entertained is comparatively few. And this Mr Darwin, with his usual fairness, frankly admits. "It may be asked," says he, "how is it that varieties which I have called incipient species become ultimately converted into good and distinct species, *which in most cases obviously differ from each other more than do the varieties of the same species;*" and he sets himself to account for this, but does not dispute the fact. He gives no instance of any wild plant or animal, subject to no restriction as to intermixture, having within the knowledge of man deviated into a well-established constant form, which would be admitted as a species by naturalists. He refers to Mr Buckman's experiments, as showing the extent of variation capable of being assumed by plants; but, on the one hand, these experiments may be classed as instances of artificial selection; and on the other, I hear (I have no personal knowledge on the subject) that there is considerable difference of opinion among botanists as to their trustworthiness. And even although they were trustworthy, their result is merely to show how various the modifications are which take place under altered circumstances, a fact which I do not deny. A writer in the "National Review" offers to supplement such instances by quoting from M. Roulin two naturally modified breeds of cattle descended from the cattle of the Pampas, and now found in the hottest parts of South America; one of them called *Pelones*, and the other *Calougas;* the former possessing a very scanty fine fur, and the latter without any hair at all, and each peculiar to the district it inhabits, and either not transferable, or with difficulty transferable, into any colder region. He thinks that these would be admitted as species by naturalists. Far from it. It is merely a simple case of modification to suit altered condition of life. It is exactly the same case in oxen as we see in the Merino and Australian sheep; but such a variation is not what we desiderate. Show us an animal between the ox and the sheep, or rather a series of animals exhibiting the transitions between them. But Mr Darwin, in reply, tells us, that we cannot expect to trace these new species in their actual transit. While commencing their variation, we call them varieties; when they are farther removed, we dispute which they are; when they are complete, we call them species. He with some justice (but not entire justice) remarks, that we are here, as compared with the great spaces of time which he requires for the development of his new species, merely at a single point of view, and at no one point can you expect to see a passage taking place, because the assumption is that every passage is gradual. We see the present species; but we do not know that we either see its parent or its descendant. I admit that, under such premises, we cannot see the passage; but surely over the whole surface of the earth, and out of all the living creatures swarming upon it, we ought to detect some species whose parents have not yet perished, and whose descendants have already appeared. Mr Darwin would like to escape from this position—but he cannot. He says "It should always be borne in mind what sort of intermediate forms must on my theory have formerly existed. I have found it difficult, when looking at any two species, to avoid picturing to myself forms *directly* intermediate between them. But this is a wholly false view; we should always look for forms—*intermediate* between each species, and a common but unknown progenitor" (p. 280). Now this is merely confusing the thing; the process being gradual, there must be some exactly and directly true half-way intermediate form between the parent species and the descendant species, and it matters not to us that we know only one of these, nor does it matter that we know neither. What concerns us is, that there ought to be half-way steps between every form and something else which is either now living, or which has lived, on the face of the earth, and if such do exist, we ought to find,—I do not say all,—but certainly many, or at least some of these. So far as regards the present age, Mr Darwin apologises for the absence of such intermediate forms, by supposing migrations to have taken place over large continuous areas, and the links to have been lost in the intermediate districts from unsuitableness of condition, or from geological changes having submerged certain districts, when, of course, the links existing here would be lost, and concludes a very specious and plausible argument on this head thus:—"Lastly, looking not to any one time, but to all time, if my theory be true, numberless intermediate varieties, linking most closely all the species of the same group together, must assuredly have existed; but the very process of natural selection constantly tends to exterminate the parent forms and the intermediate links, consequently evidence of their former existence could be found only amongst fossil remains" (pp. 177–9). Now, so far as these explanations are merely an answer to the question, Why we do not find such intermediate passages in any one particular portion of the globe? they might be accepted as an apology for their absence; but when applied to the whole of it, and to such myriads of creatures as inhabit it, it seems beyond all reasonable application of the doctrine of chances to accept it as a sufficient or even probable explanation. The very essence of the new theory is gradual passage, and slow descent by natural generation and inheritance—the parent species and the incipient species both subsisting at the same time, and the process of substitution being gradual and protracted. Mr Darwin's own map of divergence, and the whole of his reasoning go to show how parent forms, and descendant and collateral forms, may all subsist and be going on in different localities and climates at the same time. It will not therefore do to say that the new varieties developed by natural selection "continually take the place of, and exterminate their parent forms," and so prevent the occurrence of innumerable intermediate links everywhere throughout nature. But supposing that, for the sake of argument, we allow this apology for the moment, at least it can only apply to the present age of nature, or to some one definite period—it cannot also apply to past ages, or to any two or more consecutive ages; and Mr Darwin, as we have just seen, admits that "evidence of their former existence should be found amongst fossil remains" (p. 280). Are fossil remains of these then found? Is there any evidence in support of this to be drawn from fossil remains? Mr Darwin himself shall answer the question. "Why, then," says he, "is not every geological formation and every stratum full of such intermediate links? Geology *assuredly does not reveal any such finely graduated organic chain*; and this perhaps is the most obvious and gravest objection which can be urged against my theory" (p. 280). And a very grave objection it certainly is, that in the only two quarters where actual proof of facts (which must exist if the theory be true) can be sought for, and where, *à priori*, they might reasonably have been expected to be found, namely, the present and the past, they should be absent, or at least undiscoverable.

Those who are new to the subject may naturally be puzzled to guess how he escapes from such an embarrassing dilemma. The solution is abundantly simple. "The explanation," says he, "lies, as I believe, in the extreme imperfection of the geological record." Now I believe no one will dispute as an abstract proposition the extreme imperfection of the geological record. But I cannot admit that its imperfection is of that character or degree that will entitle Mr Darwin to plead it in his favour. He dwells on the poorness of our palæontological collections—the great spaces of time wholly, or nearly wholly, unrepresented in them—the extreme rarity of terrestrial animals in the deposits—the destruction of the soft parts of most animals, and the crushed state of many others. I shall not follow him into his details on these points. All that he says on the subject may be very true—is very true—but will avail him nought if, in any portion of the geological records, we can find any one succession of strata of moderate depth which may be fairly held to have been deposited unintermittently, and in which we find a liberal representation of the animals of any one class. And such records many of the enormous deposits of limestone rocks beyond doubt are—their whole phenomena indicating an uninterrupted period of tranquil deposition, extending over ages beyond our numbering, and the strata themselves bearing in their bosoms an excellent report on the molluscous animals of the period.* I have quite sufficient to test Mr Darwin's apology, if I have, first, a lengthened and uninterrupted period of deposit, and next, the marine fossils in any one class of that period well represented in it. If I am told that such representation in fossils is not only imperfect as regards terrestrial and soft animals, but also as regards molluscous shell-bearing animals, I shall go to issue upon the point, and, I think, prove that we ought to have, and do have, as good a knowledge of what were the species of shell-bearing molluscs which lived in the seas which produced many of our fossil deposits, as we have of those living at the bottom of our own seas at the present day; and no one will say that that knowledge is very imperfect. A moment's consideration of the respective means we have of knowing each will show the probability of this. The only means we have of knowing the species in our present seas is by dredging, or by the still more imperfect system of picking up those shells which may be cast ashore. Now, dredging is a mere scraping of a little morsel of the bottom of the sea here and there; and yet, by adding up the accumulated observations made in various quarters, we have arrived at a most accurate knowledge of the inhabitants of those seas which have been examined. Some shells remain rare, others unique, but this does not prevent us believing in the accuracy of our knowledge. Compare this scraping here and there in the dark, with the deliberate open-day examinations which we can make of most geological strata; miles upon miles of coast cliffs—transverse sections in ravines—and piece by piece manipulation in quarries and mines—and I think it must be admitted, that so far as that class of animals which *can* be preserved in deposits goes, it cannot be said that our knowledge of them in continuous strata is imperfect; and as, therefore, we should there find the intervening links between older and younger species if they existed, and yet do not find them, the inevitable inference is that they do not exist.

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* Mr Darwin himself remarks, that "two palæontologists, whose opinion is worthy of much deference, namely, Bronn and Woodward, have concluded that the average duration of each formation is twice or thrice as long as the average duration of specific forms" (p. 293). This opinion may be well-founded or not—I imagine it is; but it is difficult of application to the point at issue, on account of the real or possible intermissions which may have taken place in these formations.

Untenable as they appear to be, however, these arguments or apologies have satisfied Mr Darwin, and his system of natural *variation* being once admitted or held as proved, the remaining steps to natural *selection* are easy. The most essential, and one as to which I do not suppose there can be any difference of opinion, is founded on what he calls the struggle for existence. That such a struggle is constantly going on is familiar to us all; but, as I neither dispute its existence nor its bearing (always supposing his other premises to be correct), I shall not make any remarks upon it, or on some of the other minor branches of his argument, such as sexual selection, divergence of character, the effects of use and disuse, acclimatization, laws of variation and geographical distribution (on all of which, had I space and time, I see much which I feel disposed to modify or controvert). These must either stand or fall with the other parts of his theory already noticed, or do not come into direct collision with the more vital objections which I have to it. There is, however, one topic still remaining which I should not wish so to pass over—viz., the effect of physical condition in influencing form originally, and affecting it afterwards. On this point Mr Darwin and I are widely at issue. , He believes that it has had little or no influence upon them. "Neither the similarity nor the dissimilarity of the inhabitants of various regions," says he, "can be accounted for by their climatal and other physical conditions." To me, again, it appears that the effect of physical condition is one of the most powerful agents in determining the form of organic creatures; and I must be pardoned if I devote a few sentences to this part of the subject, because I look upon it as of the greatest importance, and ranking in the same category and scarcely less powerful as a proof of design on the part of the Creator than that drawn from the anatomical structure of the animal frame. Some of the instances bearing on it, given by Mr Darwin, are certainly difficult of explanation; but then, how little do we know of what the real essence of physical condition is! Look at North America, which in temperature and many other respects has a physical condition not greatly differing from our own. See how the inappreciable difference in physical condition is telling upon the white race there; the women in youth retaining the normal beauty of their race, but becoming prematurely old; the men becoming thin and sallow; the teeth decaying more rapidly; the average duration of life diminishing,—besides various other tokens of unsuitableness of climate. We all know very well that Bangalore, Darjeeling, and other hill stations in India, are mere expedients, better than nothing, but that the only real remedy for the sick Englishman is home,—home to the native physical condition. Seeing, then, that the essential part of physical condition is something of so subtle and undetectable a nature, is it a fair estimate of its effect to say, as Mr Darwin does, that certain large tracts in South Africa and South America are placed under like physical conditions; therefore, if there is any value in them, show us like product?

What! South America with its cordillera, and weeping Southern Archipelago, under similar physical conditions with arid South Africa? Even the unassisted eye can see that this is not so. But Mr Darwin chooses a test by which I am willing to abide (and which I had indeed selected for the same purpose in a paper I read on the Disguises of Nature at the meeting of the British Association at Aberdeen). It is the blind cave animals found in the limestone caverns both in Europe and America. Mr Darwin says, "It is difficult to imagine conditions of life more similar than deep limestone caverns under a nearly similar climate; so that on the common view of the blind animals having been separately created for the American and European caverns, close similarity in the organisation and affinities might have been expected; but as Schiodte and others have remarked, this is not the case, and the cave insects* of the two continents are not more closely allied than might have been anticipated from the general resemblance of the other inhabitants of North America and Europe." Now Mr Darwin, in this passage, has quite mistaken the gist of Schiodte's remark, and consequently misapplied it. It is quite correct for him to say that we should expect close similarity in the caves in question, but it is incorrect to say that "this is not the case;" for the similarity in some is marvellously close; and it is also incorrect to say that Schiodte and others have remarked that "this is not the case." As to the "others," indeed, I cannot speak, for I do not know to whom he refers, and I do not know any other author than Schiodte except Müller, who has written, from original observation, otherwise than incidentally upon the subject; but neither he nor Schiodte make any such remark. I presume the others alluded to by Mr Darwin are those who have followed Schiodte, and adopted or quoted his remark. The remark which he makes, and Mr Darwin has misapplied, is, "that the cave insects of the two continents are not more closely allied than might have been anticipated from the general resemblance of the other inhabitants of North America and Europe;"—a loose general remark, which, like an ancient oracle, may be read either way. Darwin (a disbeliever in the effects of physical condition), we see, reads it that the resemblance is slight; Schiodte, on the other hand, who appears to be a thorough believer in its effects, sees nothing more in the marvellous resemblance than might have been anticipated. I say that he seems to be a thorough believer, because he goes so far as to separate those insects which are found in stalactite caves from those in other caverns, maintaining (and I believe with justice) that the two kinds were respectively confined to these classes of caves. But let us see how the fact actually stands as to resemblance. I shall take the eyeless Anophthalmi. It is not the only one which would suit me, but it is the most striking. Although belonging to the family of Trechidæ, it possesses very marked and distinctive characters, besides the want of eyes. Nothing comes very close to it. It stands out and apart, and can be distinguished in a moment. It is found nowhere but deep in limestone caverns; but this generic form is repeated by *different species* in almost every cave which has been examined. In the caves of Adelsberg in Carniola, the two species *Anophthalmus Schmidtii* and *Anophthalmus Bilimekii* are found. *Anophthalmus hirtus* and *Anophthalmus Hacquetii*, in the Grotto of Krimberg in Oberiggdorf; *Anophthalmus Scopolii*, in the Grotto of Setz in Corinthia; *Anophthalmus Doriæ* in the Grotto des Ours in Eastern Liguria. *Anophthalmus Ghiliani* has been taken in a cavern at Monte Viso, near the French frontiers. *Anophthalmus Gallicus* and *Anophthalmus Pandellei* in the Grotto of Betharram in the low Pyrenees; *Anophthalmus Crypticola* and *Anophthalmus Orcinus*, in the Grotto of Gargas, high Pyrenees; *Anophthalmus Raymondi* in two caves near Marseilles; and *Anophthalmus Tellkampfii* in the Mammoth Caves of Kentucky, all confined to their own caves, or districts of caves, and found nowhere else. Now, how is this? When I first became acquainted with Mr Darwin's theory, it was from the perusal of the short notice of its main elements, published about eighteen months ago in the Linnean Society's Proceedings; and the imperfect account of it there given induced me to suppose that he held that every species was descended from the one nearest to it, and hence, to infer that he would hold that all the *Anophthalmi* were connected one with the other by direct descent, and I imagined that the fact of closely allied species being found in the caves of Kentucky and the caves of Carniola, without any means of communication with each other, must be fatal to his theory; but now that I understand it more correctly, I see that this difficulty can be got over by referring the two (as diverging descendants) to some common ancestor, not eyeless, who may have lived where the Atlantic Ocean now rolls, at some distant period when America and Europe were united. Therefore, the fact has not the significance I supposed. A *Trechus* may have wandered into each of these caves, and by process of natural selection, after frequent variation into all manner of other forms, which being unsuitable have not been preserved, have at last hit upon the form of an *Anophthalmus.* But if he thus saves his theory, what becomes of his disbelief in the effects of physical condition? If it has no effect, why have they all turned into *Anophthalmi?* The only explication which I can imagine for him is, that in every cave *Trechi* entered, and in each and all threw off descendants of all different kinds, as well as *Anophthalmi*, none of which were suited to the physical condition except the *Anophthalmi*, and therefore the latter alone survived. Whether this is a more philosophical explanation of their presence than the view that their production was influenced by the physical condition of the place, I leave to the reader to determine.

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* Although Mr Darwin here uses the observations of Schiodte upon blind insects as an illustration, his remarks (as he himself has had the kindness to inform me) are not meant to be confined to them, but also to be applied to the whole of the animals found in caves. But as his theory, if true, should meet every case, a clear flaw in even one would be fatal to the whole, and I would have tested it with these insects, whether they had been referred to by Mr Darwin himself or not.

I shall only follow Mr Darwin for a few lines farther in his remarks on this subject. He says, "On my view, we must suppose that American animals having ordinary power of vision slowly migrated by successive generations from the outer world into the deeper and deeper recesses of the Kentucky Caves, as did European animals into the caves of Europe. We have had some evidence of this gradation of habit; for, as Schiodte remarks, 'animals not far remote from ordinary forms prepare the transition from light to darkness. Next follow those that are constructed for twilight; and last of all, those destined for total darkness.'" If Darwin reads this as meaning that there is a gradation in form and affinity between the animals which are found at the entrance, and those found in total darkness, he is in error—there is none. It is the gradation in adaptation to darkness that Schiodte is speaking of. Those at the entrance, with small eyes, belong to the *Pristonychi*, large black beetles found in cellars and such places. The *Anophthalmi* belong to the small *Trechidæ*, of which there are none specially found at the entrance.

I might take other exception to the facts adduced by Mr Darwin, or to his application of them. For instance, he says of the woodpecker, "Can a more striking instance of adaptation be given than that of a woodpecker for climbing trees, and for seizing insects in the chinks of the bark? Yet in North America there are woodpeckers which feed largely on fruit, and others with elongated wings which chase insects on the wing; and on the plains of La Plata, where not a tree grows, there is a woodpecker, *which in every essential part of its organisation*, even in its colouring, in the harsh tone of its voice, and undulatory flight, told me plainly of its close blood relationship to our common species; yet it is a woodpecker which never climbs a tree." (P. 184.) I have selected this instance both as a statement bearing upon the effect of physical condition which appears to me to require correction, and also as an illustration of the necessity, in such an investigation as this, of testing every fact before admitting it. This is a statement made upon Mr Darwin's own personal observation, confirming that of Azara. I do not believe there is a more upright and truthful man in Britain than Mr Darwin, and yet we look at things from such an opposite point of view, that I not only do not see what he avers in the above instance, but see quite the reverse. The woodpeckers he refers to are Colaptes (the La Plata species is, I believe, the *Colaptes campestris*); and so far from appearing to me to possess every essential point of the organisation of a woodpecker, they are one of the very instances which I have been in use to give as showing the alteration of structure in a type consequent upon different physical conditions of life. The Colaptes, although allied to the woodpeckers, differ from them in mode of life, inasmuch as they feed upon ants; and, in structure, inasmuch as not requiring that most essential part of the organisation of a woodpecker (its peculiar hammering bill and strong tail) they do not possess them, while they retain the peculiar tongue and accessory muscles still necessary for securing their insect food.

The strongest points in favour of the general results come to by Mr Darwin, are a class of facts which can scarcely be said to bear distinctively on his theory more than upon various other theories already promulgated, and more or less adopted. One of these is the fact, that all animals, and all plants, throughout all time and space, should be related to each other in group subordinate to group. Another not less formidable fact is the existence of the same homological parts in different animals, sometimes aborted, and sometimes largely developed.

These are two of the great difficulties attendant on the view of the independent creation of each individual species. But although they *were* fatal to that view, it does not fall to Mr Darwin as sole Œdipus to solve them. The doctrine of progressive development (to which Mr Darwin's view has many points of affinity), or any doctrine in which development of species *ex ovo* plays a part, will explain these facts equally well. The germ must bear some trace of its origin; and hence we should, under such a theory, see not only the relationships and homologies referred to, but also certain appearances which bear indications of reversion to type, such as the appearance of the stripes of the tiger in the young of the lion, &c. These, I own, are difficult to be explained (I do not say unexplainable) under the theory of independent creation, but natural, and to be expected, under any theory of development *ex ovo*,—not more under Mr Darwin's than under any other. The distinctive character of Mr Darwin's theory is not development *ex ovo;* that is the theory of Oken, of Agassiz, of the author of the "Vestiges of Creation;" nay, I may go farther back. It is the theory of Bonnet and of Priestley, who, however *involved* their ideas might be, still held "that all the germs of future plants, organical bodies of all kinds, and the reproducible parts of them, were really contained in the first germ." Darwin's, on the other hand, is gradual transition by slow and scarcely perceptible degrees; and, *so far as that specialty is concerned*, it has no more bearing than Oken's upon the classes of facts above referred to; and the distinction between them is not confined merely to the *modus operandi* of the process of development; it is much more material than that; it embraces the question of final causes, and bears on the very existence of design in the organic creation. The views of Agassiz and Oken do not challenge the fact of design existing in the wonderful adaptations of structure to purpose which we see everywhere displayed in living organisms. *Their* theory allowed us to retain our belief in the great argument on which the whole of natural theology is based; nay, even to place it on higher grounds, as the intelligence which performs its work by the intervention of a law or machinery designed by itself, and operating on a great scale, is superior to the intelligence which executes each individual detail directly and without such intervention. If it furnished no explanation of the causes of adaptation of structure to habit, at least it did not prevent us from holding, if we chose, that, by some unexplained means, the germ of life was supplied with such a principle of growth as, under certain physical conditions, developed itself into these adaptations. We could hold design still to be there, although its direct means of operation was shrouded from our view in the laboratory of Nature. But Mr Darwin's theory is not only opposed, but absolutely inconsistent with any such idea. The talons of the eagle have not been framed as they are by design, to seize and hold its prey. The wonderfully constructed hand of the mole was not a designed gift from the Creator, but merely some variety of the hedgehog, which had broadest paws, and, being most adapted to digging, adopted the mode of life of the mole. The implement was not made for the animal, but the animal for the implement. The assumption is, that it is not alone beneficial variations which Nature makes. She makes them in any and every way; some being profitable, others the reverse; and the reason why we find all that have ever been seen on the face of the earth beneficially endowed (that is, provided with structures which, to the unilluminated eye, indicate design) is, that only those variations which happen to have been so endowed have been preserved,—the blots which Nature made having become extinct through the preponderance of the beneficially endowed. To use Mr Darwin's words, "Natural selection is daily and hourly scrutinizing throughout the world every variation, even the slightest; rejecting that which is bad, preserving and adding up all that is good, silently and insensibly working whenever and wherever opportunity offers at the improvement of each organic being, in relation to its organic and inorganic conditions of life." (P. 84.) Now, I cannot believe in such doctrine. When I look at the anatomy of any part of the body, and see exactly the same mechanism and contrivances had recourse to which a mechanician would have used to secure similar results, I cannot bring myself to believe that it is fortuitous, or other than evidence of the presence of direct design. A belief in such design I should be most loath to surrender, and I am therefore glad that, on other grounds, viz. the legitimate result of the argument already discussed, I have come to be of opinion that Mr Darwin's theory is unsound, and that I am to be spared any collision between my inclinations and my convictions.

The following Donations to the Library were announced:—

London University Calendar. 1860. 8vo.—*From the University.*

Journal of the Proceedings of the Linnean Society. Vol. IV. No. 16. 8vo.—*From the Society.*

Bulletin de la Société des Sciences Naturelles de Neuchatel. Tome V. Part I. 1859. 8vo.—*From the Society.*

Maps of Indian Survey.—*From the Government of India.*

On a New Method of Measuring Watch-Glasses. By Alexander Bryson, F.R.S.E. 1860. 8vo.—*From the Author.*

Almanaque Nautico para 1861, Calculado de Orden de S. M. en el Observatorio de Marina de la Ciudad de San Fernando. Cadiz, 1859. 8vo.—*From the Observatory.*

The Canadian Journal of Industry, Science, and Art. January, 1860. 8vo.—*From the Canadian Institute.*

Maps, Sections, and Descriptions of the Geological Survey of Great Britain.—*From the Director-General of the Survey.*

Account of the Illness and Death of Dr William Pulteney Alison. By Patrick Newbigging, M.D., F.R.C.S.—*From the Author.*

Monday, 5th March 1860.

Dr CHRISTISON, Vice-President, in the Chair.

The following Communications were read:—

1. On the Utmost Horizontal Distance which can be Spanned by a Chain of a given Material. By Edward Sang, Esq.

The method of investigation followed in this paper for finding the limit for a uniform chain, is to suppose a multitude of catenaries all having a common vertex, and to take, in each of them, that point at which the tension is fixed. The curve passing through all of these points has for its equation—

$$z = s\,(1 - \sin\theta),$$
$$x = s\ .\ \sin\theta\ .\ \text{nep log cot}\,\frac{\theta}{2},$$

in which s is the tension, z the vertical, and x the horizontal ordinate.

This curve, a correct figure of which was exhibited, has its extreme horizontal limit when $\theta=33°, 32', 03''$, from which the utmost horizontal span of a uniform chain is found to be $s \times 1{\cdot}325\,4838$, the height or versed sine being $\times 447\,5659$, and the length of the chain $s \times 1{\cdot}667\,1130$ when s is taken equal to the modulus of strength of the material.

This, however, does not show the absolute limit; for, by reducing the thickness of the chain at each point till it be just able to bear the strain to which it is subjected, we shall remove all redundant weight. The form which such a chain assumes may be called the catenary of regulated strength.

Its equations are—

$$i=\frac{x}{s}$$

$$z=s\ .\ \text{nep log sec}\ \frac{x}{s}$$

$$l=s\ .\ \text{nep log tan}\left(\frac{\pi}{4}+\frac{x}{2s}\right)$$

i being the inclination of the curve to the horizon. This curve has two vertical asymptotes placed at the distance πs from each other, which distance is thus the absolute extreme horizontal span that can be reached by a chain having s for its modulus of strength. A correct drawing of this curve was also shown.

2. On the Climate of Edinburgh for Fifty-six Years, from 1795 to 1850, deduced principally from Mr Adie's Observations; with an Account of other and earlier Registers.—On the Climate of Dunfermline, from the Registers of the late Rev. Henry Fergus. By Professor J. D. Forbes.

The paper on the Climate of Edinburgh is divided into seven sections.

The *First Section* includes an account of the earliest records of the thermometer at Edinburgh which are to be found in the *Edinburgh Medical Essays*. They date from 1731. They were made with a thermometer having an arbitrary scale, which is described in *Martine's Essays*, and they appear to have been recorded with much care; but they cease in 1736. The next series, printed in the *Essays of the Philosophical Society of Edinburgh*, commence in 1764, and are continued till 1770. A parallel Register was begun at Hawkhill, near Edinburgh, and continued till 1776, and probably later. Professor Playfair's Observations (printed in the Royal Society Transactions) only supply one year (1794) of the interval which elapses before we enter upon the elaborate Register kept by the late Mr Adie and his family.

Second Section. Mr Adie's Observations were continued from 1795 to the middle of 1805. They then ceased until the year 1821, after which they were steadily pursued, with the assistance of different members of his family, until 1850. In order to supply the missing years 1805–1820, the author was fortunate enough to recover (through the kind agency of Mr David Laing) a Register kept at Dunfermline for above thirty years by the late Rev. Henry Fergus, whose son, the Rev. John Fergus, kindly lent the Register, and allowed him to make use of it.*

The history of this Register and its results are given in a short separate paper. The climate of Dunfermline has a very close approximation in character to that of Edinburgh, not only as regards the mean annual temperature, but also as to the distribution of heat at different seasons.

By availing himself of the Dunfermline Register for the years 1805–1820, and using simple reductions, the author is enabled to estimate with considerable confidence the mean temperature of each year, and each month of each year, from 1795 to 1850, at Edinburgh.

The *Third Section* contains the monthly means of the entire series in a tabular form. The highest mean annual temperature (49°·60) was that of 1846, the lowest (44°·44) that of 1799. The mean of the whole period, deduced from nearly 35,000 observations, was 46°·77, or, excluding the Dunfermline observations, 46°·88. A series of Tables is also given, classifying the seasons according to the annual range, and by the temperature of the hottest and coldest months respectively. The hottest month was July 1808, the coldest,

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* Mr Fergus has, since this paper was read, presented this interesting MS register to the Royal Society.

January 1814. In 56 years June was 5 times the hottest month, July 36 times, and August 15 times. November was twice the coldest month of winter, March 1½ times, February 10½ times,* December 15, and January 27 times. A classification of the years according to the earliness or lateness of the greatest summer heat is next given.

Section Fourth contains the monthly and annual fall of rain from Mr Adie's observations, viz. from 1795 to 1804, and from 1822 to 1849. The mean annual fall is exactly 25 inches, ranging from 36·60 in 1795 to 15·27 in 1826. The distribution of rain in the different seasons is then given, being greatest in summer and least in spring.

In *Section Fifth*, the author has considered whether any law can be traced in the succession of the seasons throughout the period embraced by these observations. But beyond the fact, that hot and cold years usually occur in groups of from 7 to 12 years' duration, nothing definite can be deduced from the Tables. Conformably with this remark, there is a slight appearance of maximum temperatures occurring about the years 1809, 1829, and 1849; and of minima in 1799, 1819, 1839.

Section Sixth is on the form of the Annual Curve of Temperature and its Fluctuations. Adopting the usual mode of notation by the sines of arcs, and making the given date reckoned from January 0 (the extent of the year being denoted by 360°), we have for the temperature, y, of the given epoch x.

$$y = 46^\circ{\cdot}88 - 10^\circ{\cdot}98 \sin(x + 68^\circ\ 28') + 0^\circ{\cdot}96 \sin(2x + 22^\circ).$$

The average temperature of each day of the year being taken for the 40 years of Mr Adie's observations, and being projected in a curve, is well represented on the whole by the preceding formula. It is plain, however, that even 40 years is by much too short a period to give with accuracy the mean temperature of any given day. The following Table contains the mean temperature of each day of the year, founded on Mr Adie's observations.

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* When the temperature of February and March was the same, as in 1807, then one-half is the proportion for each.

Table showing the Mean Temperature of every day of the Year, at Edinburgh, from an Average of 40 *Years' observations.*

	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
	°	°	°	°	°	°	°	°	°	°	°	°
1	36·1	37·2	39·1	42·5	48·5	53·6	57·1	58·4	56·0	51·2	45·1	39·7
2	35·2	37·2	39·2	43·1	47·9	54·5	57·2	58·3	56·9	50·9	44·9	39·4
3	36·0	37·5	40·0	42·4	47·9	53·9	57·3	57·6	55·6	51·1	43·4	39·2
4	37·5	36·9	38·9	42·5	48·1	54·2	58·2	58·0	55·2	50·4	41·7	39·1
5	36·7	37·2	38·4	43·5	48·6	54·1	58·3	57·6	54·7	49·8	42·1	38·5
6	36·4	36·6	38·4	43·5	47·9	54·3	57·7	58·2	55·1	49·6	42·3	38·7
7	36·2	37·2	39·0	44·2	48·5	54·7	57·4	58·6	55·1	50·1	42·6	39·7
8	36·0	38·2	39·4	44·2	48·6	54·6	57·8	59·2	54·5	49·1	42·1	39·2
9	35·4	38·5	40·3	43·4	47·7	54·4	57·2	58·5	54·9	48·6	41·9	40·2
10	35·3	38·6	40·5	43·4	47·9	55·1	58·2	58·2	54·6	48·7	42·3	40·1
11	34·8	38·5	40·3	42·6	48·4	55·6	58·3	58·6	54·6	48·7	42·2	40·0
12	35·5	38·3	40·3	43·9	48·4	55·5	58·7	58·4	53·7	47·0	41·6	39·7
13	36·4	37·7	40·6	44·2	48·7	56·3	59·0	57·8	54·2	46·8	42·0	39·7
14	36·1	38·5	40·9	45·4	48·3	55·8	59·0	57·2	54·1	47·8	41·3	39·3
15	37·1	38·3	41·2	45·7	49·1	55·9	58·6	57·5	54·6	47·4	40·6	40·2
16	36·1	38·3	40·7	45·3	50·7	56·1	59·0	56·9	55·7	47·5	40·9	40·5
17	36·1	37·9	40·9	44·8	50·7	56·4	58·6	57·4	55·1	47·5	41·7	39·6
18	37·9	37·4	41·4	45·0	50·7	56·2	58·4	58·2	53·5	46·5	39·8	39·0
19	38·0	37·9	41·5	45·6	50·3	55·4	58·3	58·1	53·2	47·1	40·4	39·0
20	37·0	38·0	41·4	45·7	51·0	56·2	57·9	57·6	52·2	47·7	40·8	38·1
21	37·3	37·1	41·7	45·3	51·5	56·8	57·9	56·9	52·2	46·5	41·0	38·1
22	37·0	38·8	41·6	45·8	52·0	55·9	58·4	55·7	51·6	46·8	40·3	38·5
23	38·1	38·9	40·9	45·3	52·5	56·7	58·3	57·0	52·0	46·3	39·9	36·5
24	38·1	39·0	40·6	45·1	53·2	56·3	57·6	56·2	52·5	46·0	39·7	37·0
25	37·6	38·2	41·0	45·9	52·4	56·2	58·5	56·5	52·4	45·5	38·4	37·7
26	38·1	37·8	40·9	47·0	52·7	56·2	58·7	56·1	52·5	44·2	39·5	37·0
27	36·9	38·2	42·7	46·8	53·0	57·0	59·4	56·5	52·2	45·0	39·1	36·1
28	37·4	38·3	42·1	47·3	52·5	57·4	59·8	56·0	51·4	44·1	39·0	35·5
29	36·6	39·4	41·5	47·5	53·3	57·3	58·4	56·8	50·6	43·8	40·1	36·7
30	37·7	……	40·9	48·1	53·4	57·2	58·1	56·6	51·0	44·9	39·5	37·9
31	36·9	……	42·5	……	53·9	……	59·0	55·9	……	44·8	……	36·8

In this section are farther considered the inflections or "periodic anomalies" of the annual curve. When the normal curve deduced from the preceding equation (p. 295) is projected and compared with that which the projection of the daily temperatures produces, the latter is found to fluctuate considerably, and to pass in an irregular manner, sometimes above, and sometimes below, the geometric curve, which, however, fairly represents the totality of the observations.

We find, however, that there are deviations from the average or normal curve, which occur at certain seasons, and which do not appear to be entirely accidental. Several of these may be traced, for example, in each of the four decennial periods into which the series may be divided. These are called "periodic anomalies;" and their occurrence has previously been announced by De Humboldt, Arago, M. Quetelet, and others.

The most conspicuous of these anomalies occur in December, January, and February. An irregular elevation of temperature usually happens in the two middle weeks of December, followed early in January by an accession of cold, which accelerates the epoch of lowest temperature by at least a week, when we compare it with the geometric curve. This is again succeeded, in the latter part of January and beginning of February, by a period of comparative warmth. These anomalies seem to obtain at least over a great part of the west of Europe.

The author gives the name of "fluctuation" to the variation in the temperature of a given day of the year, from one year to another, arising from causes purely local and temporary, or, as we may call them, accidental. By applying the calculus of probability to the forty years' observations, we might assign the "probable uncertainty" in the determination of the temperature of any given day. The author has, however, confined himself to noting the highest and lowest mean temperature on a given day which has occurred during 40 years. These differences are sometimes very large. But they vary from one season to another according to a well-marked law. The "fluctuation" is greatest in January, when it amounts to 28° or 29°, and least in July, when it is only 16° or 17°. On the contrary, the *diurnal* range or difference of the maximum and minimum reading in 24 hours is least in December (9°·5), and greatest in June (18°).

Section Seventh.—The author concludes the paper with a comparison between the meteorological character of the seasons for fifty-six years, and the price of oats in the Edinburgh market for the same period, which was obligingly furnished to him by Mr Lawson. He has been quite unable to trace any connection between these classes of facts, and he recommends the subject to the consideration of those who are now occupied in considering the bearings of meteorology upon agriculture. When the seasons from 1795 to 1850 are arranged according to the price of oats, their order bears no intelligible relation to one or any of the previous classifications of those years according to warmth, moisture, or earliness. Thus much appears from the inquiry, that the attempt of Sir William Herschel to deduce the climatic influence of the solar spots from the market price of wheat, rests on a fallacious basis. It is only fair to add, that Sir William Herschel employed it with a reservation as to its reliability as a criterion of the temperature.

3. On the Mountain Limestone and Lower Carboniferous Rocks of the Fifeshire Coast. By the Rev. Thomas Brown. Communicated by Dr Allman.

The writer stated that this paper was the result of observations made while staying at the sea-coast for a few weeks in autumn. Near Ardross a bed of limestone had been discovered, with peculiar fossils, which promised geological results of some interest. To determine its stratigraphical position, it was necessary to reduce to order a portion of the coast, hitherto held to be in a state of hopeless confusion. This led to fuller inquiries, till the rocks underlying the coal-field had been examined from Burntisland to St Andrews.

Section I. A general description of the rocks was given, as seen along the shore, accompanied by a section in which they were laid down to scale.

Section II. Two classes of trap-rocks were referred to, viz.—1. The contemporaneous and interstratified; 2. The intrusive—this term being designedly used as expressing no opinion in regard to their origin, merely that the surrounding strata had been fractured, and through these fractures the traps in question had come into their present position. No proof had presented itself that these intrusive traps had exerted an upheaving agency, except perhaps at one point, and there only to a small extent.

Section III. The Mountain Limestone was described as consisting of three parts, viz.—

1. The six upper limestones, A to F. These, with their intercalated strata, immediately underlie the coal-fields. They are marine, and to them the term Mountain Limestone has usually been restricted. Their fossils were described.—Of crustaceans, there were one species of Trilobite, one of Eurypterus, one of Gampsonyx, (a genus not hitherto found in Britain), and two of Dithyrocaris, both, it is believed, new. Of fish, besides Rhyzodus, Holoptychius, &c.

there were two new species, a Cochliodus and a Ctenacanthus. The plates also of a tuberculated fish, belonging either to Pterichthys or some allied genus, were exhibited, proving that this great class had passed far up into the carboniferous system.

2. Estuarine strata, between F and L. These comprise the well-known Burntisland limestones, corresponding with beds west of Pittenweem.

One remarkable jaw of a pycnodont fish was exhibited from this part of the series near Kilwinning. Only one example of this class of fish-remains had previously occurred in the whole Palæozoic system—a small jaw found near Leeds.

3. The limestone L, the line of Lower Encrinites. This occurs east of Pittenweem, at Crail and St Andrews. The fossils were described similar as a whole to those of the beds A F.

Forty species of fossil shells occurring in these rocks were enumerated, only twelve of which are given in Professor Nicol's list of Scottish fossils. Among those here added, were *Sanguinolites tricostatus, Chemnitzia gracilis, Murchisonia trilineata*, &c. &c.

Section IV. Lower Carboniferous. This great underlying series was described in its leading features, especially—1. The Myalina beds—limestones composed of a single bivalve allied to Unio. 2. The deep-sea character of the fossils found at different levels, species of Orthoceras, Natica, Lingula, &c. 3. A limestone charged with great abundance of a new annelid, a species of Spirorbis, beautifully curved in a serpentine form.

Section V. Results. The two groups.

The writer explained that he had been led to deviate from the usually received classification of these rocks. Taking the upper portion of what Mr Maclaren terms the calciferous sandstones down as far as the bed L, and adding these to the upper zone, usually called the Mountain Limestone, two well-marked groups would be formed, and a well-defined line of division obtained.

1. *The Mountain Limestone.* The bed L, containing the same fossils with the six upper limestones, must (notwithstanding the intervening Estuarine beds) be classed along with them, and forms the base line of the upper group. The whole strata, from A to L, are approximately about 1000 feet in thickness. The limit upwards was not examined. The fossils all belong to the Mountain Limestone, and are extremely characteristic.

2. The underlying group, the Lower Carboniferous, is characterised by—1. The great Myalina beds. 2. The relative abundance of Cyclopteris ; the Sphenopteris being the specially characteristic plant of the upper group. 3. The circumstance that the carboniferous fauna occurs only in an incipient state. The few species of Orthoceras, Aviculo-pecten, &c., which do occur, are of a carboniferous character, and prove the presence of deep-sea conditions ; but all that specially marks the Mountain Limestone fauna (the Encrinites, Corals, Brachiopods) are either absent, or occur very scantily.

It would thus appear, that in this great series, which exhibits with singular clearness the development of the carboniferous epoch, the bed L marks the point of time when the Mountain Limestone fauna in its full strength came into view, while the lower carboniferous shows the same fauna in an incipient state. The latter may be studied with singular advantage on these shores, especially from Fife Ness to beyond Kingbarns.

The following Donations to the Library were announced :—

Memoires de la Société des Sciences Naturelles de Neuchatel. Tome IV. 1859. 4to.—*From the Society.*

Nederlandsch Kruidkundig Archief. Vol. IV., Part IV. 1859. 8vo.

Gelehrte Anzeigen. Band XLVIII. Munich. 1859. 4to.—*From the Royal Bavarian Academy.*

Jahrbuch des Kaiserlich, Königlichen Geologischen Reichsanstalt. Nos. 1 and 2. 1859. 8vo.—*From the Institute.*

Silliman's American Journal of Science and Arts. No. 85. January, 1860. 8vo.—*From the Editors.*

Atti dell' I. R. Istituto Lombardo di Scienze, Lettere, ed Arti. Vol. I. Fasc. 15 and 16. Milan. 1859.—*From the Institute.*

Memoire dell' I. R. Istituto Lombardo di Scienze, Lettere ed Arti. Vol. VIII. Fasc. 1. Milan. 1859.—*From the Institute.*

Ansprache Gehalten am Schlusse des Ersten Decenniums der Kaiserlich Königlichen Geologischen Reichsanstalt in Wien. Von W. Haidinger. 1859. 8vo.—*From the Institute.*

Die Fossilen Mollusken des Tertiær-Bekens von Wien. Von Dr Moritz Höres. Band II.—*From the Same.*

Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Nos. 25 and 26. 1859–1860.—*From the Academy.*

Journal of the Asiatic Society of Bengal. No. 4. 1859. 8vo.—*From the Society.*

Quarterly Journal of the Geological Society. Nos. 60* and 61. 1860. 8vo.—*From the Society.*

Preisschriften gekrönt und herausgegeben von der Fürstlich Jablonowskischen Gesellschaft zu Leipzig. No. 7. 1859. 8vo. —*From the Society.*

Erinnerungen an Johann Georg von Lori. Von Dr Georg T. von Rudhart. Munich. 1859. 4to.—*From the Royal Bavarian Academy.*

The Journal of Agriculture and Transactions of the Highland and Agricultural Society of Scotland. No. 68. March, 1860. 8vo.—*From the Society.*

Journal of the Statistical Society of London. Vol. XXIII. Part I. March, 1860. 8vo.—*From the Society.*

The Australian and Californian Gold Discoveries, and their Probable Consequences. By Patrick James Stirling, F.R.S.E. Edinburgh, 1853. 8vo.—*From the Author.*

Harmonies of Political Economy. By Frédéric Bastiat. Translated from the French, by Patrick James Stirling, F.R.S.E. London, 1860. 8vo.—*From the Translator.*

Monday, 19th March 1860.

Dr CHRISTISON, Vice-President, in the Chair.

The following Communications were read:—

1. On an Apprehended Depreciation of Money, arising from New Supplies of Gold. By Patrick James Stirling, F.R.S.E.

The annual production of gold, in Europe, America, and Northern Asia, at the beginning of the present century, which was estimated by Baron Humboldt in round numbers at something over two and a half millions sterling, continued at about the same rate until additions began to be received from the renewed working of the Russian mines. These additions, which, for the period intervening between 1810 and 1822, are estimated by M. Michel Chevalier not to have exceeded in all one million and a half, gradually increased, until in 1847 they amounted to upwards of four millions per annum.

In the end of 1847, auriferous deposits of unequalled richness were discovered in California. In the succeeding year, about one million was received from this new source. Next year, the supply from the same source increased to four millions; in 1850, to twelve millions; and in 1857, to fifteen and a half millions sterling.

In February 1851, rich gold-fields were discovered in the territory of New South Wales; and in the August following still richer, and apparently inexhaustible, deposits were found in the Province of Victoria. In 1857, the annual produce of that province alone amounted to nearly eleven millions.

Thus, the total annual supply of gold, which at the beginning of the century, and for several years afterwards, did not much exceed two and a half millions, has now come to average about thirty-seven millions sterling.

The entire stock of gold in Europe and America, which was estimated at the beginning of this century at 354 millions sterling, is supposed in 1848 to have amounted to about 560 millions. America, in the 356 years which intervened between the memorable voyage of Columbus and the first discovery of gold on the Sacramento, had furnished of this quantity about 400 millions. In the twelve years from 1848 to 1860, the additions made to the stock amount to no less a sum (assuming the rate of yield of 1858 to have been continued in 1859) than 280 millions,—equal to one-half of the previously existing stock. In other words, one year's supply at present is nearly equal to one-tenth of the entire American supply for three centuries and a half.

When the produce of the Russian mines had reached four millions per annum, the late Sir R. Peel expressed an apprehension that so unusual a supply might create an action on general prices; and it is not to be wondered at that the vastly increased supplies of the last twelve years should have given rise to inquiry and controversy.

In France, the question of probable depreciation was felt to be more urgent and pressing than with us, from the circumstance that the Government of that country has persisted, in defiance of all sound principle, in maintaining a double standard of money, which we had long before got rid of. The silver coins of France, as had been predicted by M. Chevalier, began soon to disappear, and have since been exported, it is believed, to the amount of not less than sixty millions sterling, equal to one-half of what was supposed to be the entire French metallic circulation.

With us the question was, whether the new and greatly enlarged supplies of that metal, which since 1816 has formed the sole standard of our money—the universal medium of exchange, the common measure of commerce, in relation to which all pecuniary contracts are adjusted, and the value of all commodities estimated—a metal, too, (unlike silver) found in a comparatively pure state, and produced at a much less expense of labour and capital,—whether these supplies, if continued, as they seemed likely to be, would produce a depreciation of money, or, what is the same thing, a universal rise in the money value of labour and commodities, affecting in this way all contracts having reference to the future, and the purchasing power of fixed incomes.

Such a disturbance of the relations of property, it was remembered, had already occurred in our history. Three centuries ago, silver was the universal standard of money in Europe. Before the discovery of the rich mines of Potosi, and the introduction of Medina's process for separating silver from its ores by amalgamation with mercury, wheat in England was sold for 6s. 8d. a quarter; beef at 1d. a pound; ale at 1d. a gallon; and claret at 3d. a quart. The wages of a mason or carpenter were 4d. a day, and of a day-labourer 2d.; while a clergyman received L.5 a year, and an Oxford scholar 10d. a week. After these discoveries, and as the direct consequence of them, wages and general prices—not in England alone, but in France, in Spain, and generally over Europe—were quadrupled, and continued at the higher level ever afterwards. This remarkable phenomenon occurred about the year 1572. There was no war, or commercial disturbance, or other accidental circumstance to account for it. The coinage had been reformed twenty years before, and brought to very nearly our present standard, and there was neither dearth nor scarcity in the land; for the old chronicler tells us, " there was no want to him who wanted not money."* The value of labour and commodities appeared to have universally risen; but in reality it was the value of money which had fallen. The change was not in the things valued, but in the measure of value.

The wise councillors of Queen Elizabeth had not shut their eyes to the changes which were then impending, and their political and social consequences. The revenues of the Universities of Oxford and Cambridge, and of the Colleges of Eton and Winchester, arising principally from land, were anciently payable in money. The Lord Treasurer Burleigh, and other far-seeing men of that day, became sensible of the great diminution of the value of money which was about to be the consequence of the increased American supplies of silver, and to elude the effect of the impending depreciation, they had an act of Parliament passed, by which it was enacted, that in all future leases to be granted by the several colleges, one third part at least of the old rent should be reserved in corn, and should be converted into money each year, according to the average market-prices of grain. The money arising from this corn-rent, although originally but a third of the whole, is now worth double the other two thirds; and thus, by the wise precautions and foresight of the great men of that time, were these venerable seats of learning prevented from falling into comparative poverty and decay.

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* True Relation of the most Remarkable Dearths, &c.

The question which now presents itself for solution is this: Are the recent discoveries of gold (now the standard of our money), and the vast accessions now making to the previous stock of that metal, producing at present, or are they likely to produce hereafter, effects similar in kind, although, it may be, inferior in degree, to the effects produced in the sixteenth century by the discovery of the American mines, and the great subsequent fall in the cost of producing silver? Or, on the contrary, are the situation of the world, and the circumstances of society, now so totally different as to place us altogether beyond the danger of any such revolution in the relations of property as our ancestors experienced in the days of the Tudors?

This subject gave rise to much controversy in 1852, after the first accounts of the Australian discoveries had reached this country. On reviewing the arguments on both sides (of which the author gave a summary), many of those whose attention had been previously directed to the subject, both here and on the Continent, felt themselves constrained to conclude that the preponderance of probability (to say the least of it) was on the side of those who maintained the likelihood of an ultimate general rise of wages and prices in Europe.

Of course, at that early period, all reasonings with reference to the future were, and could only be, conjectural, But judging from the past, and the effects formerly produced by the American supplies of silver, it was concluded that if the Australian and Californian production of gold were continued at anything like the rate it then promised, we should sooner or later witness the following among other striking effects, viz.—(1.) A marked and sudden rise of wages and prices in the gold-producing countries themselves ; (2.) A general and sustained rise, to a greater or less extent, of wages and prices in Great Britain, the United States, and other countries which employ gold as the exclusive standard of their money ; (3.) In countries, like France, which maintain a double standard of gold and silver (that is to say, where both metals are equally *legal tender*, and interchangeable, in certain fixed proportions), a continued disappearance of silver, and the gradual substitution of gold, followed by a rise of wages and prices in such countries ; (4.) A great and unusual impetus given to trade in all departments, and a marked increase of exports and imports ; (5.) No direct effect on the rate of interest, but a felt diminution in the purchasing power of fixed pecuniary incomes ; (*Lastly*) It was pointed out that these anticipated changes would probably be more or less retarded by the time necessary for the displacement of silver, and that the tendency to dearness would in some degree be counteracted by increased facilities of communication, improvements in agriculture, improved machinery, and the discovery and adoption of cheaper processes in manufactures.

Whether any or all of these anticipated results have as yet actually exhibited themselves, has given rise to much difference of opinion. Ten years are perhaps too short a period to enable us to come to a decided conclusion ; and within the last ten years several events have occurred to render the problem more complex, and its solution, for the present, more difficult ;—among others, the unusual drain of silver to the East, the displacement of silver in France, the demonetization of that metal in Holland and in our own East Indian possessions,—above all, the free-trade legislation inaugurated by Sir R. Peel, and the commercial and monetary crisis of 1857. From 1851 to 1857 the rise of wages and prices had been continuous. After the crisis a reaction took place, from the effects of which we are only now recovering, although it is believed that, in all departments of trade and industry, wages and prices are higher now (1860) than they were anterior to the Australian discoveries.

On the *first* of the probable effects which have been indicated, it is unnecessary to enlarge—the rise of prices in the gold-producing countries was marked and immediate. In California, the prices which prevailed in 1848 and 1849 were fabulous; and in Australia the rise of prices in 1851 was equally striking. From facts so exceptional no sound inference can be drawn.

A more difficult, and to us a more interesting and important subject of inquiry, is the action of the new gold supplies here at home—in the gold-receiving countries.

The price of grain, although the best available basis for instituting a comparison of prices from century to century, is perhaps the very worst on which to found such a comparison from year to year, or even for a period extending over a limited number of years. We may notice it, however, for what it is worth, during the period we are considering. Taking the average price of wheat in England for the "farmers' years," ending at Michaelmas, as given by Mr Willich, the Actuary of the Tithe Commutation Commissioners, the prices of the last eight years ended Michaelmas 1859, exceed those of the previous eight years, ended Michaelmas 1851, by nearly 12 per cent.

In provisions and butchers' meat, the rise in the London markets (as in those of Paris) has been continuous and progressive, exceeding at present the average prices of 1851 by from 20 to 30 per cent.

In colonial and tropical produce, tea, sugar, coffee, tobacco, silk, and cotton, there has likewise been an advance, although not to so great an extent; while in most of the materials of our manufactures—flax, timber, wool, and especially in metals, tallow, and some kinds of oils and dye-stuffs—there was a general and progressive rise, until the prices of 1857 exceeded those of 1851 by from 30 to 60 per cent. Consequent on the crisis of 1857 there was a sudden fall, but in very few instances to nearly so low a range of prices as prevailed in 1851; while since 1858 there has again been in most articles a continuous rise—in some instances to the extent of from 40 to 50 per cent. over the prices of 1851.

In July 1853, Mr Newmarch, in his able brochure "On the New Supplies of Gold," gave a list of the prices of 38 leading articles from the London Prices Current, selected impartially for the purpose of showing, that while many of the articles quoted had advanced in price, several of them, and these of great importance, such as sugar, tobacco, cotton, and silk, had either declined or remained stationary. I have since then kept a copy of this list, with blank columns, in which I have continued to fill in the prices every quarter from the same Price Current, in the same way as had been done for the preceding two years and a half. The result brought out in this way is, that, in the face of greatly enlarged production, and constantly increasing importations, with one or two exceptions *all* the 38 articles inserted have risen in price—most of them very materially, and many of them continuously and progressively, though, as might be expected, the constantly advancing tide has in the majority of instances been marked by fluctuations, exhibited most prominently after the crisis of 1857.

Now, this rise of prices during the last nine years (for the list begins in January 1851, and ends in January 1860) would be a serious look-out, especially for the working classes, were it not accompanied, as it appears to have been, by a simultaneous and corresponding advance in the money wages of all kinds of labour, both agricultural and manufacturing, skilled and unskilled.

Communications from eminent agriculturists represent the wages both of farm-servants and of day-labourers as having risen since 1850 fully 25 per cent. In Edinburgh, masons' wages, which in 1852 were 20s. per week of sixty hours, are now 5d. per hour, equal to 25s. a week, or an advance of 25 per cent. Joiners' wages, which in 1852 were 18s., are now 23s., or 27 per cent. higher. Upholsterers, then 19s., are now 23s., or 21 per cent. higher.

From an important paper, "On the Money Rate of Wages in Glasgow and the West of Scotland," communicated by Dr Strang to the Cheltenham Meeting of the British Association in 1856, and founded on actual returns, it appears that the increase of wages in 1856 over the rates of 1850–51 had been, in the case of masons, carpenters, and joiners, 20 per cent.; of unskilled labourers in the building trades, 48 per cent.; of engineers, 17 per cent.; of quarriers, 30 per cent.; of cotton-spinners, 25 per cent.; of power-loom weavers, 15 per cent. In a subsequent paper, communicated by the same gentleman to the meeting of the British Association at Leeds in 1858, he states, as the result of his more recent inquiries, that although the rate of wages in 1858 was lower than in 1856, it was still higher than in 1851. In a Report on the Statistics of Glasgow for 1859, Dr Strang states that "the wages of labour, now given for a week of diminished hours, have greatly advanced, and are still on the rise; and when measured either in grain or in time, are generally higher than they ever were known to be."

The price of labour, especially of unskilled labour, is perhaps the surest test we can apply in judging of the comparative value of money at different periods.

That the rate of wages in the gold-producing countries should be acted on directly by the conditions under which the material of money is produced is not to be wondered at. The manner in which it acts on wages in Europe is not so obvious, but is by no means incapable of explanation. The price of labour is subject to the law of supply and demand, depending as it does on the numbers of the working population on the one hand, compared with the fund for the payment of wages on the other. If the gold discoveries, then, have caused unusual emigration, and so thinned the labour-market, while the equally unprecedented impetus given to trade, and attributable in part at least to these discoveries, has greatly increased the fund for the employment of labour, we discover a twofold cause for the rise which has taken place.

The average annual emigration from the United Kingdom, which for the five years ending 1846 had been 87,850, increased for the five years ending 1855 to 299,300.

The increase of the fund for the payment of wages may be judged of from the unparalleled extension of trade which has taken place since 1848. The real value of the exports of British produce, which in 1848 amounted to L.52,849,445, rose in 1857 to L.122,066,107, and, according to the Board of Trade tables recently issued, after falling in 1858 to L.116,608,756, reached in 1859 the absolutely unprecedented sum of L.130,440,427. No doubt much of this increase must be set down to the credit of our recent free-trade legislation; but a considerable proportion of the increase, especially since 1851, consists of additions to our trade with the gold-producing countries. Our exports to Australia, for example, which in 1848 amounted to less than one million and a half, had increased in 1859 to eleven millions and a quarter.

The coinage of gold in Great Britain, France, and the United States, has kept pace with the increased supplies of that metal. In this country, between 1848 and the end of 1856, we had coined gold to the value of fifty millions. In the United States, during the same period, the gold coinage amounted to the value of 68½ millions sterling; while in France, between 1850 and the end of 1858, it amounted to a sum equal to L.129,587,735 sterling. The importance of these figures will be appreciated when it is remembered that during the forty-five years which preceded 1848, France had coined gold at the rate of less than one million sterling per annum—less than is now issued each year by the Royal Mint of Sydney—and that the coinage of England during the seven years preceding 1850 amounted to little more than four millions per annum.

Assuming that these unprecedented additions to the stock of gold in circulation must sooner or later depreciate the value of money, we should naturally proceed next to inquire what the effects of such a depreciation are likely to be on the condition of different classes of society; in what manner, and to what extent, it may affect the interest of landlords and tenants, debtors and creditors, labourers and capitalists, annuitants, shareholders, stipendiaries, families prospectively dependent on policies of life assurance, and others having an interest not only in the present but the future value of money; what precautions such impending changes should suggest with reference to investments; what legislative measures, if any, they invite; what are likely to be their effects in lightening the pressure of our public debt, or increasing the exactions of Government for the current service of the State; whether, and to what extent, these changes are likely to be modified or retarded by countervailing causes; and how far the tendency to dearness caused by a depreciation of money may not be compensated by the tendency to cheapness which improvements in agriculture, in manufactures, and in the various departments of industry and production, never fail to carry in their train. These are inquiries of great interest and importance, but they open up too wide a field to be entered on at present. The more immediate question is—Are there indications that a depreciation of money is imminent, or has it already actually begun?

2. On the Chronology of the Trap-Rocks of Scotland. By Archibald Geikie, F.G.S., of the Geological Survey of Great Britain. Communicated by Robert Chambers, Esq.

The first part of this paper contained a review of the existing nomenclature of trappean-rocks, and the following arrangement was given as that which the author had found to be most useful for practice in the field:—1*st*, Ash and volcanic conglomerate. 2*d*, Interbedded augitic traps (greenstones and basalts). 3*d*, Interbedded felspathic traps (felstones, porphyries). 4*th*, Intrusive augitic traps. *5th*, Intrusive felspathic traps. By adopting a different colour for each of these classes, the general relations of an intricate trappean district could be shown at a glance.

Although it was well known that the trap-rocks of Scotland belonged to several distinct geological epochs, much still remained to be done, both in determining their exact age and in working out the details of their structure. The author had been engaged in this subject for several years, and the present paper was intended as the first of a series elucidatory of Scottish trappean geology.

Silurian.—It was remarked that, both in the Lower Silurian grits of the Lammermuirs, and in the Upper Silurian grits of the Pentlands, there is an abundance of felspathic matter, pointing to the existence of felspathic rocks, either then or previously ejected. The Lammermuir chain is likewise traversed by innumerable felstone dykes, probably produced at the time of the folding of the Silurians. At Reston, in Berwickshire, beds of ash occur in the Lower Silurian.

Old Red Sandstone.—The author referred to a previous paper (read before the Geological Society) in which he had shown that the Old Red Sandstone of the south of Scotland consists of two distinct portions—one conformable with the Upper Silurian, and traversed by the same foldings and dykes ; the other lying utterly unconformably, both on the Upper Silurian and the Lower Old Red. The igneous rocks of the older series probably occur in the Sidlaw Hills ; those of the newer series are well displayed in the Pentlands, the structure of which was detailed.

Carboniferous.—The great abundance, the variety of character, the local nature, and the great vertical range of the carboniferous traps of central Scotland, was shown by a detailed sketch of the geology of the Lothians. The range of hilly ground between Bathgate and Linlithgow was pointed out as an eminently characteristic district. A careful survey had shown that no well-marked zone of the 6000 or 8000 feet of carboniferous strata in some part of the Lothians was without traces of contemporaneous igneous eruptions.

Oolitic.—The structure of Skye and Raasay was described. These islands (when examined along a section from Dun Can, in Raasay, to Dunvegan, in Skye) consist of successive sheets of greenstone, with intercalated seams of estuary limestone, shell, sandstone, and coal, belonging to the Oolitic series.

Secondary, or Tertiary.—Allusion was made to the Tertiary basalts of Mull, and to the possibility of there being other igneous rocks of that age on the mainland. A section of Arthur's Seat was exhibited, showing a series of volcanic eruptions, resting quite unconformably upon some of Carboniferous date. This later group must be greatly posterior to that below; and the author collected evidence to show that it may be regarded as later Secondary, or older Tertiary.

Letter from the Rev. Dr Livingstone, F.R.S., to Dr Lyon Playfair, C.B.

River Shire, *28th Oct.* 1859.

My dear Dr Playfair,—We left England in April 1858, and up to this time we have not received a single private letter from home. This saves me the trouble of apologising to any of my friends whom I have neglected. So here goes into the middle of things. We have just traced this river up to its point of emergence from the hitherto undiscovered Lake of Nyassa or Nyinyessi. This discovery is of more importance than at first sight appears, for it opens a cotton-field superior, I imagine, to the American, inasmuch as there are no frosts to endanger or cut off the crops; and instead of the unmerciful toil required to raise the staple there, one sowing of foreign, probably American seed, introduced into several parts by the natives themselves, serves for three years' crops. Even when burned down, the plants spring up fresh again. It may have disadvantages to counterbalance these points in its favour, but of these I am at present ignorant. There is a good day's channel from the sea at *Kongone* harbour up to Murchison's Cataracts in lat. 15° 55′ south. We have then only 33 miles of cataracts, past which a common road could easily be made—and the Shire itself is again navigable right into Nyassa in lat. 14° 55′ south. Above the cataracts the land is arranged into terraces east of the river. The lower or Shire Valley is about 1200 feet high, and exactly like the valley of the Nile at Cairo, only a little broader. The second terrace is over 2000 feet and three or four miles broad; the third over 3000, or about equal to Table Mountain at the Cape (long spoken of as the highest in South Africa). We travelled in the hottest season of the year, or that called in the West the "smokes," when, from the burning of tens of thousands of acres of tall grass, the atmosphere becomes like a partial cloud or fog, but insufferably hot. When we ascended the second terrace, the air felt delightfully cool, and on the third it was perfect, neither too cold nor too hot. All these terraces are wonderfully well supplied with running rills of deliciously cool water; and cotton of the indigenous variety (which feels more like wool than cotton, and requires to be cropped annually) is cultivated to a very considerable extent. On the last terrace rises Mount Zomba, which we ascended, and found to be in round numbers 7000 to 8000 feet high. Here it was cold; but there is cultivation and a fine stream in a large valley on its top. It has a base of 20 or 30 miles. We have thus differences of climate within a few miles of each other. This for keeping Europeans well. Then we are indulging the pleasant belief, from which you may deduct a percentage, that we can cure the fever, even in the lowlands, quickly and without loss of strength to the patient. We shall beat Holloway yet! only we tell every one what our pills are made of. It is the system followed when I was alone, and adverted to at the end of my book. We have, thank God, not lost a man yet, and gave the quinine a fair trial. It never prevented an attack. We have given it up now. We took it after our wine was done, partly for the sake of the dram, and partly to prevent you folks blaming us after we were dead.

Well, beyond Zomba the land between Shire and the two lakes of Shirwa, or, as its name really is, Tamandua, and Nyassa or Nyinyessi, contracts into a narrow isthmus, and all the slave trade from the interior must cross, in order to get past the lakes without embarking on either. We met a large slaving party there. I think they are what people suppose to be Arabs at the Angoxia river, but they could not speak Arabic. They were the most blackguard-looking set I ever saw. When they understood we were English, they made off by night, probably with the same opinion of us as we had of them. They had an immense number of slaves. A station for lawful commerce here would root out that traffic.

The lake at its southern end seemed eight or ten miles broad—had a heavy swell on it though there was no wind—and it must be large to give off the Shire constantly (80 to 120 yards wide, 2 fathoms deep, and 2½ knot current) the whole year, with a variation in the river of about 2 feet from the wet to the dry season.

We should have explored it, but had left Mr Macgregor Laird's vessel in a sinking state. Funnel, furnace, deck, and bottom, all became honeycombed simultaneously. . . .

David Livingstone.

The following Donations to the Library were announced:—

Quarterly Return of Births, Deaths, and Marriages registered in Scotland—Quarter ending 31st December 1859. 8vo.—*From the Registrar-General.*

Comptes Rendus hebdomadaires des Séances de l'Académie des Sciences. Nos. 9, 10. 4to.—*From the Academy.*

Monthly Notices of Astronomical Society. Vol. XX., No. 4. 8vo. —*From the Society.*

List of Fellows of the Royal Astronomical Society, February 10, 1860. 8vo.—*From the same.*

Journal of Proceedings of Linnean Society. Supplement to Vol. IV.—Botany. 8vo.—*From the Society.*

Monthly Return of Births, Deaths, and Marriages registered in Scotland—January and February 1860. 8vo.—*From the Registrar-General.*

Answer to Sir David Brewster's Reply to Messrs Stevenson's Pamphlet on Sir David Brewster's Memorial to the Treasury. By D. and T. Stevenson. Edinburgh, 1860. 8vo.—*From the Authors.*

Descriptions of New Palæozoic Fossils from the Western States. By J. H. M'Chesney. Chicago, 1859. 8vo.—*From the Author.*

Proceedings of the Royal Geographical Society of London. Vol. IV., Part I. 1860. 8vo.—*From the Society.*

Maury's Sailing Directions. 2 vols. 1858. 4to.—*From the United States Government.*

Maury's Wind and Current Charts.—*From the same.*

Part of a Panoramic View of Kashmir Mountains. Sketched by T. G. Montgomerie, Bengal Engineers, to illustrate G. T. Survey of Kashmir.—*From Prof. C. P. Smyth.*

Fac-simile of an Ancient Manuscript on Parchment in the Record Office, which has been copied and printed by Photo-Zincography at the Ordnance Survey Office.—*From Colonel James.*

Monday, 2d April 1860.

Lord NEAVES, Vice-President, in the Chair.

The following communications were read:—

1. On the Solidification of Limes and Cements. By George Robertson, C.E.

The paper was deduced from a series of experiments, made on the London Dock Works with blue lias and Dorking limes; and at Leith Docks with the limes in the neighbourhood of Edinburgh, and with cements both natural and artificial. A short account was first given of the nature of the experiments at the London Docks, with the points investigated. A longer and more professional account of these is to be found in the Transactions of the Institution of Civil Engineers for 1857–58.

The solidification of lime was considered under two general heads.

1*st*, That of ordinary limes and plasters, in which the action of setting was proved to be at first entirely of a mechanical nature, when slaked lime is used for mortar; when quick lime is used, though the set is hastened by the chemical act of hydration, it is still, in practice, caused by mechanical causes,—the set in both cases being due to the absorption into the pores of the lime of a great part of the water of mixture, accompanied by a contraction in the bulk of the mortar. After the first set, induration increases; at first from the evaporation of some of the water of mixture, and afterwards from the absorption of carbonic acid from the atmosphere. The author found, that when limestones were cemented together, the mortar absorbed carbonic acid from the stones themselves, somewhat in proportion to the quantity in the limestone. He also found that the presence of silica in chemical combination with lime deadened the desire of the lime for carbonic acid. In hydraulic lime, therefore, which owes the permanence of its set to silica in combination with it, the induration due to carbonic acid is less than in ordinary and purer limes.

The second division of the subject comprised all limes and cements which set *permanently* hard under water. In this case, though the first set is caused by the same absorption of the water of mixture as in ordinary limes, the permanent hardness cannot, evidently, result from desiccation, either from absorption or evaporation. A chemical combination between the lime and some agent capable of resisting the softening action of water is necessary. Silica was proved to be the only substance found naturally in limes, which was capable of furnishing this protection; and *it* was only useful when present in combination with alumina, as clay, in a hydrated form. Double silicates of lime and alumina form harder mortars than silicate of lime alone. Iron, manganese, magnesia, and alumina by itself, were shown to have no chemical effect in setting lime under water, though they furnish a kind of temporary mechanical protection from solubility. The iron in pozzuolana may even be withdrawn by an acid, without to any extent injuring the hydraulic properties of the earth. The author believed that in properly calcined hydraulic lime no union between the silica and lime took place in the kiln, but only on the completion of a hydrate by slaking, and then only when surplus water was present. This theory was founded on experiments upon the adhesion of mortar at different stages of setting, silicate of lime having little power of cementation. The degree of adhesion, marked tolerably exactly the progress of the silicification of the lime. Experiments on the quantity of silica derived from Portland cement, digested in a weak acid, both when fresh from the kiln and after having been set for six days, gave still more direct evidence of the truth of this theory, which was proved by the author to be of great practical importance in the application of limes. It was stated that, oxalic acid had been ascertained to be capable of setting limes permanently hard under water, when mixed with the water used in slaking; but the oxalate of lime does not possess the same flinty hardness as the silicate, and of course is much more expensive—at the present price of oxalic acid. The paper closed with some remarks on "cements," both natural and artificial; the properties of which, under the generic types of Roman and Portland cements, were shortly explained.

2. On Zinc-Methyl. By J. A. Wanklyn, Esq.

Considerable difficulties attend the preparation of zinc-methyl. Frankland, who discovered the body, obtained it by heating pure iodide of methyl and zinc enclosed in small glass tubes. Owing to the high temperature at which reaction takes place, much gas is formed; hence the operation must be confined to very small quantities of materials.

No determination of the boiling-point, specific gravity, nor yet of the vapour density of zinc-methyl, was made by its discoverer; from which fact may be inferred how small was the product available for investigation.

Frankland* has recently endeavoured to improve the process of preparation. He has tried a modification similar to that which he had introduced for zinc-ethyl. He mixed ether with the iodide of methyl, and heated with zinc in his copper digester. By this means ready decomposition of the organic iodide was obtained, and very little gas was evolved; but subsequently it was found impossible to separate the ether from the zinc-methyl. By this process Frankland did not succeed in obtaining any pure zinc-methyl.

To meet this difficulty is the object aimed at by the author of the paper.

Instead of using ether to mix with iodide of methyl, the author uses either a strong solution of zinc-methyl in ether, or pure zinc-methyl; either of which he has found capable of rendering the action of zinc upon the organic iodide easy, and unaccompanied by much gaseous products. All his digestions he makes in glass tubes heated in the water-bath to 100° C. The strong solution of zinc-methyl was obtained, in the first instance, by digesting together ether, iodide of methyl, and zinc, and afterwards distilling. The distillate was then employed in a second operation in place of ether, and so a still stronger solution of zinc-methyl resulted.

By repeating the process zinc-methyl was finally obtained in a state of tolerable purity. A single tube, which had undergone four digestions, furnished about half an ounce of product, which analysis showed to consist of zinc-methyl nearly pure.

A determination of the vapour density of zinc-methyl made by Gay Lussac's method gave 3·291. The calculated theoretical number is 3·299. Accordingly, the condensation of zinc-methyl corresponds to that of ether, and not to that of hydride of methyl—in this respect resembling zinc-ethyl.

In addition to the properties of zinc-methyl mentioned by Frankland, viz. its extreme inflammability, its action upon water, &c., the author has observed that the body may be heated to 200° C. without decomposition. At 270° C. it begins to yield metallic zinc.

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* Annalen der Chemie u Pharmacie von Liebig, cxi. p. 62. Frankland.

The boiling-point of zinc-methyl lies between 50° C. and 60° C., but the author reserves the accurate determination until he shall be in possession of several ounces of the pure body.

In one respect the author's observations do not accord with Frankland's; he has not been able to verify what has been advanced concerning the poisonous nature of zinc-methyl fumes.* Having performed more than a dozen distillations of the body, and having been much exposed to the fumes, he has not been able to mark any special effect upon his health. With regard to the disagreeable odour of the body, he has also to remark, that a mixture of zinc-methyl with much ether is more offensive than zinc-methyl pure, or containing only a little ether.

The author remarks, that considerations drawn from the state of condensation of the so-called organo-metallic bodies, lead to the conclusion that the metals are not the representatives of hydrogen. The formulæ of equal volumes of the following bodies reduced to a state of vapour are adduced as examples:—

Zinc-methyl.	Zinc-ethyl.	Mercury-methyl.	Mercury-ethyl.
$\left.\begin{matrix} C_2\,H_3 \\ C_2\,H_3 \end{matrix}\right\} Zn_2$;	$\left.\begin{matrix} C_4\,H_5 \\ C_4\,H_5 \end{matrix}\right\} Zn_2$;	$\left.\begin{matrix} C_2\,H_3 \\ C_2\,H_3 \end{matrix}\right\} Hg_2$;	$\left.\begin{matrix} C_4\,H_5 \\ C_4\,H_5 \end{matrix}\right\} Hg_2$.

Distann-methyl.	Distann-ethyl.
$\left.\begin{matrix} C_2\,H_3 \\ C_2\,H_3 \\ C_2\,H_3 \\ C_2\,H_3 \end{matrix}\right\} Sn_2$;	$\left.\begin{matrix} C_4\,H_5 \\ C_4\,H_5 \\ C_4\,H_5 \\ C_4\,H_5 \end{matrix}\right\} Sn_2$.

Hydride of methyl.	Hydride of ethyl.	Methyl-ether.	Ethyl-ether.
$\left.\begin{matrix} C_2\,H_3 \\ H \end{matrix}\right\}$;	$\left.\begin{matrix} C_4\,H_5 \\ H \end{matrix}\right\}$;	$\left.\begin{matrix} C_2\,H_3 \\ C_2\,H_3 \end{matrix}\right\} O_2$;	$\left.\begin{matrix} C_4\,H_5 \\ C_4\,H_5 \end{matrix}\right\} O_2$.

From which it appears that there is twice as much methyl in the standard volume of zinc-methyl vapour as there is methyl in the standard volume of hydride of methyl. It appears, also, that Zn_2 in zinc-methyl represents O_2 in methyl ether. There is no organo-metallic compound known in which the metal has a condensation corresponding to that of hydrogen.

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* Liebig's Annalen der Chemie u Pharmacie, lxxi. p. 214. Frankland.

3. Notes of the Dissection of a Female Beaver. By John Cleland, M.D., Demonstrator of Anatomy, University of Edinburgh. Communicated by Dr Douglas Maclagan.*

In this paper the writer directed attention to the remarkably developed parotid glands of the beaver, which form a single large mass covering the whole front and sides of the neck, and cannot be separated from one another. He showed how the apparent disproportion between the anterior and posterior extremities of the beaver was increased by the way in which the parietes of the abdomen overhung the thighs, and the margin of the *panniculus carnosus* passed over the knees so as to include them in the muscular investment that enveloped the trunk.

The use of the horny development, called by zoologists an additional nail, on the second toe of the hind foot, was explained thus:—the toes, especially the first and second, are curved inwards for purposes connected with the animal's aquatic habits; and when it supports itself in its favourite posture on its hind legs and tail, these two toes lie with their outer sides downwards. The innermost is small, and bears none of the weight; but the second toe bears a considerable amount, and the tender matrix of the nail would be constantly pressed against the ground, were it not that the horny development is so shaped that, while it presents its under surface to the earth, the superior aspect fits in to the concavity of the claw above.

The dilatation of the *vena cava* for diving purposes was described, and the arrangement of the castor sacs.

The principal points with regard to the latter are as follows:—The rectum and genito-urinary aperture open into a common depression as in the male, and the oil glands and general form of the castor sacs resemble those of the male; but while in the male the necks of the castor sacs of opposite sides are united by a communication above the preputial opening—*i. e.*, between the prepuce and rectum—in the female they are united by a dilatation like a small third lobe, situated, not on the superior aspect of the genito-urinary aperture as in the male, but on the pubic aspect; and the clitoris appears on the anterior margin of the dilatation immediately inferior to the urethral orifice. The castor sacs and oil glands are invested by a muscular tunic, whose fibres, arising superiorly in connection with the compressors of the rectum, pass round and are inserted on the inferior aspect of the sacs into a circular set of fibres which surround an uninvested space in the middle line, opposite the median dilatation. The round ligaments of the uterus end on this muscular tunic. The rectum is surrounded by a set of remarkably developed compressors.

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* Published in full, with a plate, in the Edinburgh New Philosophical Journal, New Series, vol. xii. p. 14.

4. On the Thyroid Gland in the Cetacea; with Observations on the Relations of the Thymus to the Thyroid in these and some other Mammals. By William Turner, M.B. (Lond.), Senior Demonstrator of Anatomy, University of Edinburgh. Communicated by Professor Goodsir.

The author, in the first instance, directed attention to the discrepant statements of various comparative anatomists respecting the thyroid gland in the Cetacea, quoting from the writings of John Hunter, Meckel, Cuvier, Carus, and Dr Martyn. He then related the result of his own dissections made on three specimens of the common porpoise (*Phocæna communis*), one being a fœtus, another a well grown male, the third an adult male. In each of these animals a well marked thyroid gland was found, lying on the anterior and lateral surfaces of the trachea at its upper end, and extending slightly upwards on each side over the outer surface of the cricoid cartilage. It presented no division into two lateral lobes, as described by Cuvier and Carus, but consisted of a single uniform mass extending across the middle line. In the adult animal, which was examined in the fresh state, the other specimens having been some time in spirits, the gland presented a dark purple tint, and a soft and somewhat succulent aspect. Both in the fœtus and well grown animal, the thymus gland was exceedingly well developed. A detailed description of the position and relations of this gland was then given, the long ascending processes which pass upwards, by the side of the great vessels, as far as the thyroid gland, being especially pointed out. These processes were intimately connected with the lateral portions of the thyroid by cellular tissue, but were not continuous with them.

The author next referred to Mr Simon's description of the thymus in the fœtal dolphin.

The microscopic characters of the thymus gland in the well-grown porpoise were then given. It was found to consist of small closely packed corpuscles, about the size of, or a little larger than, the red corpuscles of human blood—its structure, in fact, exactly corresponding with that which is familiar to us in the fœtal gland. Thus this animal gave us an additional illustration of the truth of the statement made by Haugsted and Simon, that the thymus is not merely a fœtal structure, but that it plays an important part in the animal economy for some time after birth.

The author considered that the close relation which was found in these porpoises between the thymus and thyroid glands, might be regarded as confirmatory of the view entertained by Professor Goodsir, that they are developed from a common structure.

He next described a dissection of the thymus and thyroid, which he had made in an adult male Hartebeest (*Bubalus Caama*). The thyroid in this animal was separated into two distinct lateral lobes, each lobe having connected with it a long slender glandular process, which passed down the sides and front of the trachea, behind the sternum, into the anterior mediastinum. These glandular processes exhibited microscopically the characters of the thymus, so that, both as regards structure and position, they must be regarded as constituting that gland.

The thymus and thyroid glands in the Nylghau (*Antilope picta*) were then described. The animal dissected was a magnificent specimen of an adult male, standing one foot above the recorded average height of the male. In it the thyroid was divided into two distinct lateral lobes, each lobe extending from the cricoid cartilage as far as the fourth tracheal ring. Situated on the anterior surface of the trachea, and on the crico-thyroid membrane between these thyroidean lobes, were scattered lobules of glandular tissue of a slightly reddish tint. These were not connected with the thyroid, but were lying in the cellular tissue between its lobes. Similar scattered lobules extended for some distance down the trachea, but about thirteen inches above the sternum they became aggregated together, so as to form two long lines of glandular tissue, which passed beneath the sternum into the anterior mediastinum. Structurally this gland presented the character of the thymus. It corresponded also to it in position. In addition to the proper gland structure, the microscope brought into view numerous three-sided prismatic crystals, resembling those of the triple phosphate, lying in and about the connective tissue of the gland.

From the evidence afforded by the dissection of the Hartebeest and Nylghau, the conclusion must be drawn that in these Antilopidæ the thymus is a permanent gland; for there could be no doubt but that both these animals had reached the adult period of life, and even acquired a considerable age,—their large size, and the worn appearance of the teeth rendered this sufficiently manifest. So far, then, as regards these animals, the thymus must be looked upon as possessing a more enduring function than has hitherto been ascribed to it in the economy,—not disappearing or altogether degenerating in the early period of extra-uterine life, but persisting even in the adult animal. The paper concluded by some remarks upon the thyroid and thymus glands in the human subject.

5. Notice on the Boring of the Pholadidæ. By Alexander Bryson, Esq., President of the Royal Physical Society.

In this communication the author referred to the various theories advanced to account for the boring of the Pholadidæ in rocks.

The first hypothesis, which supposes that the molluscs perforate by means of the rotation of the valves acting as augers, he disproved by exhibiting old individuals of the *Pholus crispata* with the dentated costæ as sharp as in any young specimen. That these animals bore by silicious particles secreted by the foot, as suggested by Mr Hancock, has been disproved by microscopic observation; and that currents of water set in motion by vibratile cilia, seemed also insufficient to account for the phenomenon.

Another theory supposes that an acid is secreted by the foot, capable of dissolving the rock.

This the author showed was not tenable, as the strongest Nordhausen sulphuric acid fails to dissolve aluminous shales and Silurian slates; and also that any such acid secretion would act more readily on the valves themselves. From many experiments on the cutting of hard silicious substances, the author found that the softer the substance was in which the cutting material was impacted, the greater the amount of the work done. He was thus led to the conclusion that the Pholadidæ bore with the strong muscular foot alone, and that they obtain the silica from the waves or the arenaceous rocks in which they are found; and hence there is no necessity for either an acid or silicious secretion. That the foot was the boring apparatus, and not the valves, he proved from a specimen of a Pholas hole in shale, where the pedal depression of the animal was distinctly seen.

He also exhibited a piece of glass bored to the depth of 1·50 of an inch, by means of the point of the finger and emery alone.

The following Gentlemen were balloted for, and elected Ordinary Fellows:—

George A. Jamieson, Esq., M.A.
Rev. Leonard S. Orde.
Patrick Dudgeon, Esq., of Cargen.

The following Donations to the Library were announced:—

Comptes Rendus hebdomadaires des Séances de l'Académie des Sciences. Nos. 11, 12.—*From the Academy.*

Rectification of Logarithmic Errors in the Measurements of two Sections of the Meridional Arc of India. In a Letter to Professor Stokes, Sec. R.S. By Colonel Everest, F.R.S.—*From the Author.*

Reply to Messrs Stevenson's Pamphlet on Lighthouses. By Sir David Brewster, K.H., D.C.L., F.R.S.—*From the Author.*

Bulletin de la Société de Géographie. Quatrième Série. Tome XVIII. Paris, 1859. 8vo.—*From the Society.*

Observations Astronomiques faites à l'Observatoire de Genève, dans les Années 1853 et 1854. Par E. Plantamour. XIII^e et XIV^e Série. 1859. 4to.—*From the Author.*

Resumé Météorologique de l'Année 1858, pour Genève et le Grand St Bernard. Par E. Plantamour. Geneva, 1859. 8vo.—*From the Author.*

Uber die Natürliche Lage von Bern, von B. Studer. Bern, 1859. 4to.—*From the Author.*

Mémoires de la Société de Physique et d'Histoire Naturelle de Genève. Tome XV., Part 1st. Geneva, 1859. 4to.—*From the Society.*

Abhandlungen, herausgegeben von der Senckenbergischen Naturforschenden Gesellschaft. B. III., Part 1st. 1859. 4to.—*From the Society.*

Biographical Sketch of the late Dr George Wilson. By Professor Balfour.—*From the Author.*

Die Fortschritte der Physik im Jahre, 1857. Dargestellt von der physikalischen Gesellschaft zu Berlin. XIII. Jahrgang. Zweite Abtheilung.—*From the Society.*

Magnetical and Meteorological Observations made at the Honourable East India Company's Observatory, Bombay, in the year 1857, under the superintendence of Lieutenant E. F. T. Fergusson, I.N., F.R.A.S. Bombay, 1858.—*From the Government of India.*

Lithograph of the Fossil Trees discovered in the Quarry at Granton. —*Presented by Robert Allan, Esq., F.R.S.E.*

Tuesday, 17th April 1860.

THOMAS STEVENSON, Esq., Councillor, in the Chair.

The following Communications were read:—

1. On the Birds of Linlithgowshire. By Rev. John Duns, F.R.S.E., Torphichen.

The author, having referred to the value of carefully prepared reports on the ornithology of particular districts, characterised the physical features of that part of Linlithgowshire in which his observations had mainly been made, and showed that it is well fitted for the support of a comparatively large number of species of birds. He stated that the following list had been gradually formed as the result of observations spread over fifteen years. Most of the species named had come under his own notice; for a few he had been indebted to Thomas Durham Weir, Esq. of Boghead, an accurate observer. The classification followed is that which he had found most helpful to his own studies. Species seldom met with are printed in italics.

ORDER I.—RAPTORES.

Families.	Genera.	Species.
FALCONIDÆ.	Milvus	—*M. Regalis.*
	Falco	—*F. æsalon*, F. tinnunculus.
	Accipiter	—A. nisus.
	Circus	—C. cyaneus.
STRIGIDÆ.	Strix	—S. flammea, S. aluco, S. otus, *S. brachyotus.*

ORDER II.—SCANSORES.

Families.	Genera.	Species.
CUCULIDÆ.	Cuculus	—C. canorus.

ORDER III.—PASSORES.

Families.	Genera.	Species.
A. Fissirostres.		
CAPRIMULGIDÆ.	Caprimulgus	C. Europæus.
HIRUNDINIDÆ.	Hirundo	H. rustica, H. urbica, H. riparia.
	Cypselus	C. murarius.
HALCYONIDÆ.	Alcedo	A. ispida.
B. Tenuirostres.		
CERTHIADÆ.	Certhia	C. familiaris.
	Troglodytes	T. Europæus.
C. Dentirostres.		
TURDIDÆ.	Turdus	T. merula, T. torquatus, T. pilaris, T. viscivorus, T. musicus, T. iliacus.
	Cinclus	C. Europæus.
AMPELIDÆ.	Bombycilla	B. garrula.
LANIADÆ.	Lanius	L. excubitor.
SYLVIADÆ.	Sylvia	S. rubecula, S. hortensis, S. atricapilla, S. cinerea, S. phœnicurus, *S. locustella*, S. arundinacea, *S. phragmites*.
	Phillopneuste	Ph. sylvicola, Ph. trochilus.
	Regulus	R. auricapillus.
	Saxicola	S. œnanthe, S. rubetra, S. rubicola.
	Accentor	A. modularis.
	Parus	P. cæruleus, P. major, P. longicaudatus, P. ater.
	Motacilla	M. Yarrelli, M. boarula, M. flava (Budytes Rayi).
MUSCICAPIDÆ.	Muscicapa	M. grisola, *M. luctuoso*.
D. Conirostres.		
CORVIDÆ.	Corvus	C. corone, C. cornix, C. frugilegus, C. monedula.
	Garrulus	G. melanoleuca, G. glandarius.
STURNIDÆ.	Sturnus	S. vulgarus.
	Pastor	*P. roseus*.
FRINGILLIDÆ.	Pyrgita	P. domestica.
	Fringilla	F. montifringilla, F. cœlebs, *F. spinus*, F. carduelis, F. cannabina, F. linaria, F. flavirostris, *F. borealis*.
	Loxia	L. chloris, L. pyrrhula, *L. Europœa*, *L. coccothraustes*.
	Emberiza	E. citrinella, E. miliaria, E. nivalis, E. schœniculus.
	Alauda	A. arvensis, *A. arborea*.
	Anthus	A. pratensis, *A. arboreus*.

ORDER IV.—COLUMBÆ.

Families.	Genera.	Species.
COLUMBIDÆ.	Columba	C. palumbus.

ORDER V.—GALLINÆ.

Families.	Genera.	Species.
TETRAONIDÆ.	Tetrao	T. Scoticus, *T. tetrix*.
	Perdix	P. cinerea, *P. coturnix*.
PHASIANIDÆ.	Phasianus	Ph. Colchicus.

ORDER VI.—GRALLATORES.

Families.	Genera.	Species.
ARDEADÆ.	Ardea	A. cinerea.
	Botaurus	*B. stellaris*.
CHARADRIADÆ.	Charadrius	C. hiaticula.
	Pluvialis	P. aurea, P. squatorala, *P. morinellus*.
	Vanellus	V. cristatus.
	Hæmatopus	H. ostralegus.
	Strepsilas	S. interpres.
	Calidris	C. arenaria.
SCOLOPACIDÆ.	Scolopax	S. rusticola, S. gallinago, S. gallinula.
	Numenius	N. arquata.
	Totanus	T. hypoleucus, T. calidris.
	Tringa	T. variabilis, *T. cinerea*.
RALLIDÆ.	Rallus	*R. aquaticus*.
	Crex	C. pratensis.
	Gallinula	G. chloropus.
	Fulica	F. atra.

ORDER VII.—PALMIPEDES.

Families.	Genera.	Species.
ANATIDÆ.	Anas	A. anser.
	Boschas	B. fera, B. *crecca*.
COLYMBIDÆ.	Podiceps	*P. minor*, *P. auritus*.
LARIDÆ.	Larus	L. ridibundus, L. marinus, L. canus, L. argentatus.
	Sterna	S. hirundo.

Marine Species named are to be met with on the shore of the Frith of Forth, between Grangemouth and Queensferry. In a series of Notes, the author described, at considerable length, the structure and habits of many of the Species named in the list, and referred to specimens on the table in illustration of his remarks.

2. On an unusual Drought in the Lake District in 1859. By John Davy, M.D., F.R.SS. Lond. & Edin.

This occurrence, following an unusual fall of rain in January, took place in May, June, and July. The ordinary amount of rain in these months is,—taking the average of the last eleven years,—

at Lesketh How, Ambleside, 12·36 inches; during the months in question, at the same place, it was only 4·54 inches.

Three tables are given by the author in elucidation; the first relating to the fall of rain in five different places in the district; the second affording a summary of general meteorological observations at Kendal, more or less applicable to other parts of the district; the third containing, for the sake of comparison, the rain-fall at various places in the United Kingdom.

The author concludes with noticing the abnormal state of the weather during the whole of the year, marked by great vicissitudes of wet and drought, of heat and cold, and their effects, especially on vegetation.

3. On the Constitution of the Essential Oil of Cajeput. By Mr Maximilian Schmidl, Assistant to Dr T. Anderson, University of Glasgow.

The author shows, that oil of cajeput is a mixture of an oil boiling about 175° Cent., and one or more oils of higher boiling point. In the present paper he investigates the first of those substances. When purified by repeated distillation, it is a colourless, limpid fluid, which by analysis and determination of its vapour density, is shown to have the formula $C_{20}H_{16}+2HO$. When treated with anhydrous phosphoric acid, it is decomposed, and yields a mixture of three different hydrocarbons, to which the author gives the names of Cajputene, Isocajputene, and Metacajputene. The two former, though differing in properties, have both the formula $C_{20}H_{16}$. The last, which is a very heavy oil, with a lemon yellow colour and brilliant fluorescence, is $C_{40}H_{32}$.

Monohydrate of Cajputene, $C_{20}H_{16}+HO$, is obtained by treating the original oil with commercial sulphuric acid at the boiling temperature, under particular precautions. The substance condenses to 4 volumes of vapour, although containing one atom of oxygen.

Hexhydrate of Cajputene, $C_{20}H_{16}+6HO$ is obtained by agitating the bihydrate with dilute sulphuric acid, and leaving the mixture at rest for some time. Beautiful crystals gradually form in the mixture. The hexhydrate melts at 120° and solidifies at 85°. It is soluble in boiling alcohol and ether, from which it is deposited on cooling. Another compound, the constitution of which is not yet determined, is obtained by the action of dilute nitric acid in the cold.

Bihydrochlorate of Cajputene, $C_{20}H_{16}+2HCl$ is obtained by mixing the oil with one-third of its bulk of strong aqueous hydrochloric acid, and then passing a current of the gas through the mixture ; after the lapse of 10 or 12 minutes, the whole solidifies into a mass of crystals. These, when purified by expression and crystallisation from boiling alcohol, melt at 53°, and when repeatedly distilled, or when acted on by alcoholic potash, lose one-half of hydrochloric acid, and yield the monohydrochlorate. It is entirely devoid of taste and smell, and in this respect differs remarkably from the isomeric compound obtained from oil of turpentine.

Monohydrochlorate of Cajputene, $C_{20}H_{16}+HCl$ is an oily fluid, with a pleasant ethereal odour.

Tetrabromide of Cajputene, $C_{20}H_{16}Br_4$. Bromine is added to the bihydrate, and the mixture left for some weeks, when a granular substance is seen to deposit. As soon as this is observed, the whole is dissolved in boiling alcohol, and on cooling, glittering scales, resembling cholesterin, are deposited. It is soluble in alcohol and ether, melts at 60°, and may be distilled apparently unchanged.

Hydriodate of the hydrate of Cajputene. When iodine is added to oil of Cajeput, the temperature rises, and on cooling a black crystalline compound is deposited. This substance, after purification by cold alcohol, forms black crystals fusible at 80°, and very readily decomposed. Its formula is $C_{20}H_{16}HO+HI$.

Hydriodate of Cajputene, $C_{20}H_{16}+HI$. To obtain this compound, oil of cajeput is mixed with a solution of iodine in bisulphide of carbon, and to this a solution of phosphorus, in the same menstruum, is added. A brisk reaction takes place, the temperature rising to about 80°. After some weeks fine black crystals are deposited, which are soluble in alcohol and ether, and exceedingly stable, being unaffected even by alkaline solutions.

The author proposes to make the composition of the other constituents of the oil of cajeput the subject of a future paper.

4. On the Action of Chlorine on Citric Acid. Hexachlorinated Acetone. By John Galletly, Esq.

At the recommendation of Dr Anderson, Glasgow University, I have re-examined the oil which Plantamour obtained by acting on citric acid with chlorine. Owing to the slowness with which it is formed, its complete investigation is somewhat lengthened, and the following results are all I have as yet obtained. A considerable part of the work has been done in the Glasgow College Laboratory.

Plantamour found that when a strong solution of citric acid was exposed to the action of chlorine in the sunshine, a heavy oil appeared on the surface of the liquid, gathering in drops and sinking to the bottom. He describes the properties of this oil,* and ascribes to it the formula $C_8\ Cl_8\ O_3$. In his examination of acetone,† Staedeler found that a substitution product of this body, with five atoms of chlorine replacing hydrogen, resembled Plantamour's oil in every respect, and that the percentage composition of hexachlorinated acetone corresponded exactly with Plantamour's formula. There could therefore be very little doubt left as to its composition; but as I had the following analyses made before I was aware of Staedeler's suggestion, and as Plantamour procured a potash salt from it to which he assigns a formula having eight atoms carbon, and as his own analyses were not given in the memoir, I have published the following to remove any doubt.

Upwards of twenty half-gallon bottles filled with chlorine, having a little of a saturated solution of citric acid in each, required many weeks exposure to sunshine to yield about a fluid ounce of the oil. There seemed to be no other substance formed, unless, perhaps, hydrochloric acid and water. After drying over chloride of calcium it distilled entirely about 400° Fahr. I found its density at 60° Fahr. to be 1·748. Its properties agree very exactly with Plantamour's description.

I. { 7·200 grs. substance gave
3·620 grs. carbonic acid,
·200 grs. water.

II. { 9·250 grs. substance gave
29·790 grs. chloride of silver.

	Experiments.		Theory.		
	I.	II.			
Carbon,	13·71	—	13·58	C_6	36
Chlorine,	—	79·67	80·38	Cl_6	213
Oxygen,	—	—	6·04	O_2	16
			100·00		265

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* See Rapport Annuel de Berzelius 7^e^ année.

† Nachricht. von der Gesellsch. der Wiss. Zu Göttingen. 1853. No. 9.

The following data were obtained in determining the vapour density :—

Temperature of air 20° Cent.,
,, of vapour at sealing 247° Cent.,
Excess of weight of balloon ·810 gramme,
Capacity of balloon, 165 cub. centimètres,
Residual air, . 7 ,, ,,
Barometer, 30·20 inches,
Density, 9·417 inches.

The formula $C_6\ Cl_6\ O_2$ requires 265 × ·0346 = 9·169.

This oil is not decomposed by sodium, or at least very slowly, even with heat. It is not affected by boiling with oxide of silver, oxide of mercury, or even baryta water.

When the substance is agitated with water and cooled to 6° Cent. a crystalline hydrate is formed, which, according to Plantamour, fuses at 15° Cent. = 59° Fahr. This hydrate is $C_6\ Cl_6\ O_2 + 2HO$ (Staedeler). It often appears as a net-work of long crystals on the sides of the bottles in which the oil is formed, but these crystals may sometimes be heated considerably above the fusing point given by Plantamour without melting. I found, on keeping the oil under water for some months, that it turned into an opaque white mass of crystalline plates, which did not fuse till heated to a temperature of about 40° Fahr. When these crystals were melted again under water, they solidified shortly after cooling, without the fusing point being lowered. As this does not happen when the oil is freshly made, it is probable that the hexachlorinated acetone passes into some isomeric modification like the analogous body chloral. This hydrate disolves very readily in ether in the cold, giving long crystals covering the sides of the basin as the ether evaporates. If heat be employed to dissolve the crystals, the hydrate is decomposed and the oil separates.

When gaseous ammonia is passed into hexachlorinated acetone, it becomes solid, and there is an evolution of chloroform. The solid substance was washed with water, and crystalised from alcohol, when it formed large pearly square tables. A few grains of this body left some hours in the water-bath entirely volatilises. Its character and composition agree exactly with trichloracetamide, the following numbers having been obtained on analysis :—

	Theory.	Experiment.
Carbon	14·77	14·97
Hydrogen	1·23	1·52
Chlorine	65·54	65·14
Nitrogen	8·61	8·59
Oxygen	9·85	—
	100·00	

The same body is got by using aqueous ammonia; the decomposition taking place according to the following equation :—

$$\underset{\text{Hexachlor.}}{C_6\,Cl_6\,O_2} + \underset{\text{Acetone.}}{NH_3} = \underset{\text{Chloracetamide.}}{C_4\,Cl_3\,O_2\,NH_2} + \underset{\text{Chloroform.}}{C_2\,H\,Cl_3}$$

The products of decomposition with ammonia are therefore the same as those obtained when this alkali acts on the chlorinated ethers and aldehydes. I got a similar result with potash, which differs from the experience of Plantamour, who found the salt of a new acid which he names Bichloroxalic Acid. On dissolving the oil in alcoholic potass, it became warm and deposited chloride of potassium; and the alcoholic mother liquor, when left to spontaneous evaporation, gave long silky needles containing 53·08 per cent. chlorine. This agrees exactly with trichloracetate of potass, which contains 52·85 per cent. chlorine. The decomposition being evidently

$$\underset{\text{Hexachlor.}}{C_6\,Cl_6\,O_2} + \underset{\text{Acetone.}}{KO\,HO} = \underset{\text{Trichloracetate Potas.}}{C_4\,Cl_3\,KO_4} + \underset{\text{Chloroform.}}{C_2\,H\,Cl_3}$$

The paper in which these crystals were pressed when heated with water gave a solution which abundantly reduced nitrate of silver, showing the presence of formic acid from the further decomposition of the chloroform by the alcoholic potass.

It seemed to me probable that, by treating ethylate of soda with the oil, a decomposition would ensue in which a homologue of chloroform would be obtained, the ethyl replacing the hydrogen in this body. The decomposition, however, turned out differently. A considerable quantity of trichloracetic ether was produced, easily recognised by its fragrant peculiar odour, and among other bodies, chloroform and common salt.

The remainder of my material was expended in one or two preliminary experiments on its action on the volatile organic bases. It appears that like ammonia they give ordinary chloroform, when one of the radicals is hydrogen, the compound radical entering the chloracetamide. Thus the decomposition with aniline is as follows:—

$$\underset{\text{Aniline.}}{(C_{12}H_5)\,H_2N} + \underset{\text{Hexachlor Ac.}}{C_6Cl_6O_2} = \underset{\text{Trichloracetanilide.}}{C_4Cl_3O_2NH\,(C_{12}H_5)} + \underset{\text{Chloroform.}}{C_2\,H\,Cl_3}$$

The change occurs in the cold. Trichloracetanilide crystalises in colourless prisms of some size. It is insoluble, or very sparingly soluble, in cold water, but dissolves in warm water pretty readily, filling the liquid with silky needles as the solution cools. I expected that, like chloracetamide, it would sublime without decomposition, which it partly did; but there is a great loss, and much charcoal is left. I had barely enough left to ascertain by experiment its exact composition, and the analysis of the sublimed substance gave an excess both in carbon and hydrogen, but the method of formation and its properties leave no doubt about the formula. As I intend to continue the subject, I shall probably have an opportunity of publishing an exact analysis. When rapidly distilled with strong solution of potass, or with soda lime, there is formed besides aniline a new volatile base, with a very peculiar pungent odour. This body seems rather easily decomposed, but I had far too little to be able to give a particular account of it at present.

When the oil is mixed with a base containing a triatomic radical, such as lutidine, no chloroform is evolved. The liquids mix, but there is no apparent action in the cold, and when heat is applied, the decomposition is so violent that only a charcoal-like residue is left.

The summer being the only season when the oil can be conveniently prepared by Plantmour's process, I have made some experiments with a view to find other methods of procuring it, which I may mention here. Staedeler found that chlorinated acetones were procured by distilling citric acid with chlorate of potass and hydrochloric acid. The oil which I procured in this way had a specific gravity of 1·726, and boiled between 360° and 400° F. It was evidently a mixture of his pentachlorinated and hexachlorinated acetone. It is extremely irritating, causing great pain in the eyes, and can scarcely be distilled except with special arrangement for carrying off the vapours. I procured by a few distillations a portion of fluid having very nearly the specific gravity and boiling point of Plantamour's oil, but the irritating action of its vapour was scarcely diminished. It gave likewise the solid compound when mixed with aniline, so that there is no doubt about the identity of the bodies. I distilled in the sunshine the mixture given above, in the hope that the product might be only hexachlorinated acetone, but a violent explosion put an end to the experiment. The mixture obtained in this way is evidently the same as that Plantamour got by acting with chlorine on a citrate.

The similar oil got from citric acid, oxide of manganese, and hydrochloric acid, had only a specific gravity of 1·50 and a portion boils even below 212°, but the greater part seems to be pentachlorinated acetone. From the low boiling point of some of the fractions, it may be questioned whether these bodies are all chlorinated acetones.

The action of chlorochronic acid is too violent, and I have not been able to find a better mode of preparing the oil than the slow action of chlorine in the sunshine, which gives it pure without any trouble.

The pentachlorinated acetone decomposes with aniline in a similar manner with the body described, giving what is probably bichloracetanilide.

5. Notice of a Panoramic Sketch of Kashmir, recently received from India. By Professor C. Piazzi Smyth.

A recent panoramic topographical view of the interesting region of Kashmir having been just communicated to me by my friend, Mr Roger Montgomerie, Advocate, I have found it so remarkably well executed, in its almost photographic representation of natural features of hill and plain, that I have obtained his leave to present it to the Royal Society, accompanying the presentation with the following few particulars, abstracted from MS. reports.

The view in question is by Captain Montgomerie, B.E., who, under the general direction of Col. A. Scott Waugh, the Director of the Indian Trigonometrical Survey, has been constantly at work in Kashmir and Thibet since 1855, and with such energy and success, that he has completed the accurate triangulation, as well as the topographical surveying, of 40,000 square miles of country in four years. The whole of that extent, too, lay in a very difficult region, abounding in snowy peaks; one of which, a newly discovered point on the Kara-Koran, is probably the second highest mountain in the world; the measured height, subject only to a small probable correction, to be determined by an extensive levelling operation now in progress, being 28,278 feet.

The most important part, however, socially of the whole region visited, was the world-famous valley of Kashmir, where the splendid climate, traces of an ancient civilization, and considerable present wealth, skill, and industry, combined to add their influence to the beauties of mountain scenery.

Of the leading features in the physical geography of Kashmir Captain Montgomerie's panorama seems to give a very accurate idea; for, before the skill of the artist was called in to represent light and shade, the vertical and horizontal angles between all important points were laid down with a theodolite. In length, he describes the valley to measure one hundred and ten miles, in a S.E. and N.W. direction, with a breadth of about seventeen miles, and a mean elevation above the sea of 5200 feet. A considerable portion, or about ninety miles in length, is so nearly flat, that the river Vedusta (Jhelum), which flows through it, is navigable for that extent in large boats. The flat ground is, however, in two terraces, the upper one consisting of remains of an ancient clayey stratum, some two to three hundred feet thick. It is found often in isolated patches, several miles long and broad, with steep cliff-like edges.

The higher level thus formed was once artificially irrigated, but is now sadly neglected, while the greater part of the lower level is subject to inundation, and is more or less in a marshy condition, or absolute lake, as in the immediate neighbourhood of the capital city. The country, however, including the surrounding mountains, still contains no less than 4606 villages, spread over an extent of 8100 square miles.

On the slopes of the hills cultivated tracts are again met with alternating with grazing grounds, and forests of cedars, pines, and firs. At 7000 and 9000 feet these predominate; while above that level, and extending to 12,000 feet, there are grassy regions visited by the ponies, cows, and sheep, during half June, all July, August, and half September; these being, in fact, the only months when localities so elevated are free from snow. On the higher peaks, rising to 15,000 and 18,000 feet, snow is perpetual; and there are large glaciers between their spurs.

Even when among the smaller hills, the surveyor's labours were by no means light; and according to the extensive experience of Colonel Waugh, the Director of the Indian operations, it is not often that sufficient mathematical and artistic skill are found in any one individual, conjoined to the necessary bodily vigour; *i. e.*, to be able to clamber out of hot Himalayan valleys up the steep slopes leading to exposed and windy eminences, and there stand uninjured, while making all the necessary observations, with the full amount of attention to accuracy and fulness of detail that is proper; and even when these desirable qualities are found, they do not last, he says, in the generality of individuals more than a few seasons.

The inconveniences, however, of the lower country, and the "filling in" survey, were greatly surpassed by what was experienced during the scientific part of the triangulation, which was necessarily for the most part carried on amongst the peaks of the snowy range; out of sixteen principal stations in Thibet, fourteen being above 15,000 feet high, and two of them, Shinishah and Shunika, being respectively 18,417 and 18,224 feet. Nevertheless, no pains were spared in procuring good work, as the following extracts may best show in Captain Montgomerie's own words:—

" 12. The triangulation depends on the Kashmir series which emanates from a side of the N.W. longitudinal series in the low hills near Sealkote.

13. " In order to connect the triangulation in the Punjab with Kashmir, it was necessary to carry the triangles across the Chatadhar and Pir Paujal snowy ridges. This was done by taking observations from the tops of the snowy peaks best adapted to form a series of symmetrical polygons and quadrilaterals. In this way the triangulation has been carried on systematically from the foundation. It consists of one main axis, viz., the principal triangulation, which is composed of polygons and quadrilaterals. From this axis diverge numerous minor series of triangles, which, starting from one side of the principal series, are tested by closing on another side of the same, or upon a side of the N.W. Himalayan series.

" 14. From these minor series, secondary stations have been fixed, so as to cover the whole country with tested trigonometrical points.

" 15. Though the country to be surveyed was so elevated, the rigorous rules of the Great Trigonometrical Survey of India were adhered to throughout.

" 16. The highest points suited to the triangulation were always occupied, and observations were taken from stations upwards of 16,000 feet above the level of the sea.

" 17. On the principal series of triangles, the observations were invariably made to luminous signals, viz., heliotropes and lamps, notwithstanding the physical difficulties and the severity of the climate on the snowy peaks, so especially trying to the natives of India who served the signals.

" 18. Numerous observations being required, it was necessary for me to reside on the peaks for at least two days and nights, generally more.

" 19. Some of the peaks below 14,000 feet lose the greater part of their snow by September; but practically, it was necessary to observe from most of the stations earlier in the season, when the snow was still heavy at 11,000 feet.

" 20. Occasionally, in consequence of clouds and storms, the party had to remain pitched on the snow for upwards of a week at a time.

" 21. On the Pir Paujal peaks, the electricity was so troublesome, even when there was no storm, that it was found necessary to carry a portable lightning conductor for the protection of the theodolite.

" 22. Space sufficient, even for our very small camp, could never be got quite close to the stations on the peaks; during the day this did not matter, but at night, though the distance might not be more than 200 yards, it was rather a difficult matter to get back from the Observatory tent to the camps, after I had finished taking the observations. Soon after sunset the surface of the snow becomes as slippery as glass, affording by no means a satisfactory footing on a narrow ridge, with a precipitous slope, or a precipice, on either side."

In addition to these physical difficulties, it should be mentioned that there were others of a political kind; for Kashmir and Thibet are independent countries, and the inhabitants saw their hills ascended and encamped on by British surveyors with great distrust. Nevertheless, Captain Montgomerie displayed so much tact that he completely won the confidence and regard at last both of the Maha Rajah Golab Sing and the Maha Rajah Rumheer Sing. Then the too memorable mutiny of 1857 broke out; and while most of the captain's European assistants were withdrawn to assist in the military operations, the natives who were left with him had come from the same plains of Hindostan as the rebellious Sipahis, nevertheless all the attendants proved faithful; and except on one occasion the progress of the survey was not interfered with.

It will thus be seen, that the great Indian trigonometrical survey which began amongst the pestilential jungles of southern India, and worked gradually, northward under Lambton and Everest, has now at last reached the greatest mountain chain in the world; and never before were such great heights submitted to the processes of accurate geodesy. The occasion appears to have called forth a deal of peculiar talent among our Indian officers; and amongst those who have specially shone forth in this calling was an assistant of Captain Montgomerie's, who subsequently joined the army before Delhi and Lucknow, and was killed before the latter place by an accidental explosion of powder. He was lamented much by the surveyors, among whom he was unrivalled in physical power, endurance, and cheerfulness under fatigue; his adventures and achievements amongst the snowy mountains had been the frequent theme of praise and admiration in many an encampment; and he had intended to devote his rare and splendid qualities as a mountain surveyor, had he survived, to the exploration of central Asia on rigorous principles.

Captain Montgomerie is now actively at work, continuing in the higher regions of Thibet what he began and carried out so well in the lower valley of Kashmir.

The following Gentleman was balloted for, and duly admitted an Ordinary Fellow:—

WILLIAM CHAMBERS, Esq. of Glenormiston.

The following Donations to the Library were announced:—

Second Annual Report of the General Board of Commissioners in Lunacy for Scotland. Edin. 1860. 8vo.—*From the Commissioners.*

Journal of the Royal Asiatic Society of Great Britain and Ireland. Vol. XVII. Part II. 8vo.—*From the Society.*

Canadian Journal. March, 1860. 8vo.—*From the Canadian Institute.*

Quarterly Journal of Chemical Society. No. 48.—*From the Society.*

Appendix to Messrs Stevenson's Answer to Sir David Brewster's Reply regarding Dioptric Lights. By D. and T. Stevenson. —*From the Authors.*

Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Nos. 14 and 15.—*From the Academy.*

Von der Bedeutung der Sanskritstudien für die griechische Philologie. Von Dr W. Christ. 4to.—*From the Royal Bavarian Academy.*

Rede in der öffentlichen Sitzung der k. Akademie der Wissenschaften. Am 28. Marz 1860. Gehalten Von Justus F. Von Liebig. 4to.—*From the same.*

Proceedings of the Royal Medical and Chirurgical Society of London. Vol. III. No. 3. 1860. 8vo.—*From the Society.*

Proceedings of the Zoological Society of London. Part 3. 1859. 8vo.—*From the Society.*

Monthly Return of Births, Deaths, and Marriages. March, 1860. —*From the Registrar-General.*

Monday, 30th April 1860.

PROFESSOR CHRISTISON, Vice-President, in the Chair.

The following Communications were read :—

1. Account of the Asafœtida Plants (*Narthex Asafœtida* (Falconer), which have recently flowered and fruited in the Edinburgh Botanic Garden. By Professor Balfour.

The author gave an account of the cultivation of plants of Asafœtida in the Edinburgh Botanic Garden since the year 1842, and of the flowering and fruiting of specimens in 1858 and 1859. He then described the characters of the plant (*Narthex Asafœtida* of

Falconer), and illustrated his communication by specimens of the plant, by photographs, and by drawings from the accurate pencil of Dr Greville.

2. On the Composition of the Glassy Surface of some Vitrified Forts. By Thomas Bloxam, F.C.S., Assistant Chemist to the Industrial Museum of Scotland.

The present paper was undertaken at the suggestion of the late Professor George Wilson, as forming an interesting inquiry, both to the antiquarian and the chemist, and as a sequel to the paper upon old Scotch glass, communicated by me to this Society some time since.

The investigation had for its object,—

1*st*, To ascertain the exact composition of the glassy surfaces of the various specimens.

2*d*, The composition of the rocks upon which the glass was formed.

3*d*, To account for the formation of such glass by the information afforded by the first two inquiries.

Vitrified forts, or sites, as some prefer to call them, are spaces of ground enclosed by a wall composed of pieces of rocks placed upon each other without mortar, and the surfaces of which are covered with a glassy matter. It is scarcely the province of the chemist to speculate as to the purpose for which these sites were erected, or the manner in which the vitrification was brought about: we will therefore proceed with the inquiries set forth at the commencement.

Of the many vitrified forts existing in Scotland, two of the best were selected for investigation—the one, "Duno'deer," from Aberdeenshire, kindly forwarded by Mr Stuart of the Society of Antiquaries; the other, "Knockfarrel," from Strathpeffer, Ross-shire, procured for me by my friend Dr M. Thomson.

Both these specimens exhibited vitrification in a marked degree. The specimen from Duno'deer presented two surfaces of a vitrified nature, the one being more porous in appearance than the other; separate portions of these surfaces were carefully broken off for analysis, and distinguished as upper and under surface, the under one being the more porous. The specimen was coated with moss in parts, but these were excluded from the quantity selected for analysis. The vitrified surfaces were spread over rocks of different nature, such as coarse sandstone, and pinkish white portions of a body resembling felspar.

By a preliminary test, the powdered glassy matter was found to contain metallic iron, and where the vitrified material existed in sufficient quantity, the following points were ascertained:—

1. The specific gravity.
2. Action of alkalies and acids.
3. Chemical composition.

The specific gravity ascertained in the manner usually adopted in the laboratory was found to be 2·35.

The method used for the analysis was the same that is generally employed for silicates, the iron, as peroxide, separated from the alumina by reduction in hydrogen, and subsequent separation by permanganate of potash; the metallic iron was estimated by measuring the quantity of hydrogen evolved from a known weight of substance.

The alkalies present were estimated by fusion of the substance with baryta.

The upper surface of Duno'deer was soluble in acids to the amount of 20·46 per cent., consisting of silica, peroxide iron, protoxide of iron, alumina, and lime.

Alkalies acted upon it to a small extent, dissolving only 0·93 of a grain per cent., which was composed mainly of silica and alumina.

The quantitative composition was found to be the following:—

Silica,	46·80	
Peroxide iron,	19·92	Ratio of oxygen in the bases to that of silica as 1 : 1.
Alumina,	20·07	
Lime,	7·81	
Magnesia,	0·89	
Iron,	0·11	Oxygen quotient, 0·76.
Potash,	0·94	
Soda,	3·44	
	99·98	

The under surface of this specimen was not so uniform in appearance as the upper one, small crystals resembling felspar being scattered through it; these crystals, however, were not included in the portion taken for analysis. As the vitrified surface was smaller in extent than that of the preceding specimen, the action of acids and alkalies had to be omitted.

The specific gravity was 2·54, and the quantitative composition—

Silica,	44·21	
Peroxide iron,	19·24	Oxygen ratio as 1 : 1.
Alumina,	23·26	
Lime,	7·64	
Magnesia,	2·34	Oxygen quotient, 0·87.
Soda,	2·57	
Potash,	0·95	
Iron,	0·31	
	100·52	

The body resembling felspar was next examined; the crystals did not effervesce with acids, were easily powdered, and, when heated at the blow-pipe flame, they fused at the edges, and were easily marked with a knife.

The specific gravity was 2·37, and the chemical composition:—

Silica,	78·45	Oxygen ratio as 1 : 5.
Alumina,	13·39	
Lime,	1·48	
Peroxideiron,	1·31	Oxygen quotient, 0·209.
Potash,	1·24	
Soda,	4·90	
	100·77	

From the above analysis, the substance most resembles albite or soda-felspar; the excess of silica, and consequent reduction of the oxygen quotient, may be accounted for by the fact of its having undergone considerable weathering, and also that small quanities of quartz might have been associated with the specimen used for analysis.

The sandstone forming part of the fort was of a coarse variety, and evidently, from its appearance, had been exposed to considerable heat; it contained less silica than is usually found in building sandstones.

Analysis afforded the following results:—

Silica,	68·80
Peroxide iron,	5·00
Alumina,	19·15
Lime,	2·25
Magnesia,	0·60
Potash,	2·54
Soda,	1·66
	100·00

The last specimen of vitrified fort examined was from Knockfarrel; the vitrified portion was very distinct, but owing to the small quantity at my disposal, the preliminary experiments had to be omitted. The rocks collected in the immediate vicinity were for the most part red granite and gneiss.

The specific gravity was 2·85, and the chemical composition—

Silica, . .	54·42	
Peroxide iron, .	15·19	Oxygen ratio as 1 : 2.
Alumina, . .	19·72	
Magnesia, . .	2·49	
Lime, . .	6·42	Oxygen quotient, 0·57.
Soda, . .	0·07	
Potash, . .	0·61	
	98·92	

It will be seen by comparing the whole analyses, that the two surfaces of the vitrified fort Duno'deer, differ from each other in very slight degree, and, from the presence of alumina and soda, it appears most probable that the felspar with which it is associated has played an important part in the formation of the glass, its fusion being aided by the alkali furnished in the ashes of the wood used, the lime might also be derived from the same source.

The vitrified fort, Knockfarrel, resembles that of Duno'deer, although not closely, and from the fact of micaceous rocks being found in the vicinity, it is most likely that these rocks entered into the composition of the vitrified material.

Of the small quantity of metallic iron found in Duno'deer it perhaps is scarcely necessary to say much; but it is quite possible that it may either have existed as such in the basaltic rocks, or have been reduced from the state of oxide, a constituent of augite and olivine.

The following general conclusions may be drawn from the information afforded by the analyses:—

1*st*, That these vitrified surfaces must have been produced by the bringing together of certain rocks, and exposing them to a high temperature.

2*d*, That these rocks were not collected without regard to their relative powers of melting.

3*d*, That such a collection of rocks, basaltic, gneiss, and felspathic, would at high temperature produce a glass with wood ashes, similar in composition to these vitrifications.

I subjoin a tabulated view of the analytical results:—

	Duno'deer, upper surface.	Duno'deer, under surface.	Felspathic substance	Knockfarrel.
Silica..............	46·80	44·21	78.45	54·42
Peroxide iron.....	19·92	19·24	1·31	15·19
Alumina..........	20·07	23·26	13·39	19·72
Lime	7·81	7·64	1·48	6·42
Iron................	0·81	0·31	...	...
Potash............	0·94	0·95	1·24	0·61
Soda...............	3·44	2.57	4·90	0·07
Magnesia.........	0·89	2·34	...	2·49
	99·98	100·52	100·77	98·92
Specific gravity..	2·35	2·54	2·37	2·85
Oxygen ratio.....	1 : 1	1 : 1	1 : 5	1 : 2
Oxygen quotient.	0·76	0·87	0·20	0·57

3. On the Reduction of Observations of Underground Temperature, with applications to Professor Forbes' Edinburgh Observations and the continued Calton Hill Series. By Professor William Thomson, Glasgow.

The principle followed in the reductions which form the subject of this communication may be briefly stated thus:—

The varying temperature during a year, shown by any one of the underground thermometers on an average for a series of years, is expressed by the ordinary method in a trigonometrical series of terms representing simple harmonic variations*,—the first having a year for its period, the second a half year, the third a third part of a year, and so on. The yearly term of the series is dealt with separately for the thermometers at the different depths, the half yearly term also separately, and so on, each term being treated as if the simple periodic variation which it represents were the sole variation experienced. The elements into which the whole variation is thus analysed are examined so as to test their agreement with the elementary formulæ by which Fourier expressed the periodic variations of temperature in a bar protected from lateral conduction, and experiencing a simple harmonic variation of temperature at one end, or in an infinite solid experiencing at every point of an infinite plane through it a variation of temperature according to the same elementary law. In any locality in which the surface of the earth is sensibly plane and uniform all round to distances amounting at least to considerable multiples of the depth of the lowest thermometer, and in which the conducting power of the soil or rock below the surface is perfectly uniform to like distances round and below the thermometers, this theory must necessarily be found in excessively close agreement with the observed results. The comparison which is made in the investigations now brought forward must be regarded, therefore, not as a test of the correctness of a theory which has mathematical certainty, but as a means of finding how much the law of propagation of heat into the soil is affected by the very notable deviations from the assumed conditions of uniformity as to surface, or by possible inequalities of underground conductivity existing in the localities of observation. When those conditions of uniformity are perfectly fulfilled both by the surface and by the substance below it, the law of variation in the interior produced by a simple harmonic variation of temperature at the surface, as investigated by Fourier, may be stated in general terms in the three following propositions:—(1.) The temperature at every interior point varies according to the simple harmonic law, in a period retarded by an equal interval of time, and with an amplitude diminished in one and the same proportion, for all equal additions of depth. (2.) The absolute measure in ratio of arc to radius, for the retardation of phase, is equal to the diminution of the Napierian logarithm of the amplitude; and each of these, reckoned per unit of length as to augmentation of distance from the surface, is equal to the square root of the quotient obtained by dividing the product of the ratio of the circumference of a circle to its diameter into the thermal capacity of a unit of bulk of the solid, by the thermal conductivity of the same estimated for the period of the variation as unity of time. (3) For different periods, the retardations of phase, measured each in terms of a whole period, and the diminutions of the logarithm of the amplitude, all reckoned per unit of depth, are inversely proportional to the square roots of the periods.

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* By a simple harmonic variation is meant a variation in proportion to the height of a point which moves uniformly in a vertical circle.

The first series of observations examined by the method thus described were those instituted by Professor Forbes, and conducted under his superintendence during five years, in three localities of

Edinburgh and the immediate neighbourhood; (1.) The trap rock of Calton Hill; (2.) The sand below the soil of the Experimental Gardens; and (3.) The sandstone of Craigleith Quarry. In each place there were, besides a surface thermometer, four thermometers at the depths of 3, 6, 12, and 24 French feet respectively. The diminution in the amplitude, and the retardation of phase in going downwards, has been determined for the annual, for the half-yearly, third-yearly, and the quarterly term, on the average for these five years for each locality. The same has been determined for the average of twelve years of observation, continued on Calton Hill by the staff of the Royal Edinburgh Observatory.

The following results with reference to the annual harmonic term are selected for example:—

Average of five years, 1837 *to* 1842.

	Retardation of phase in days, per French foot of descent.	Retardation of phase in circular measure, per French foot of descent,	Diminution of Napierian logarithm of amplitude, per French foot of descent.
Calton Hill.			
3 feet to 6 feet.	......	·11635	·12625
6 „ 12 „	......	·11344	·12156
12 „ 24 „	......	·11490	·10959
Mean, or 3 to 24.	6·68 days.	·1147	.1154
Experim^{l}. *Garden.*			
3 feet to 6 feet.	......	·11635	·10037
6 „ 12 „	......	·11929	·11304
12 „ 24 „	......	·10617	·10844
Mean, or 3 to 24.	6·6 days.	·11137	·10859
Craigleith Quarry.			
3 feet to 6 feet.	......	·063995	·09372
6 „ 12 „	......	·066903	·06304
12 „ 24 „	......	·066903	·06476
Mean, or 3 to 24.	3·86 days.	·066489	·06840

If Fourier's conditions of uniformity, stated above, were fulfilled strictly, the numbers shown in the second column for each locality would be equal to one another, and equal to those in the third column. The differences between the actual numbers are surprisingly small, but are so consistent that they cannot be attributed to errors of observation. It is possible they may be due to a want of perfect agreement in the values of a degree on the different thermometric scales; but it seems more probable that they represent true discrepancies from theory, and are therefore excessively interesting, and possibly of high importance with a view to estimating the effects of inequalities of surface and of interior conductivity. The final means of the numbers in the second and third columns are

Calton Hill . . .	·11702
Experimental Garden .	·11061
Craigleith Quarry . .	·06988

The thermal capacities of specimens of the trap rock, the sand, and the sandstone of the three localities were, at the request of Professor Forbes, measured by Regnault and found to be respectively

·5283, ·3006, and ·4623.

Hence, according to proposition (3), stated above, the thermal conductivities are as follows:—

Trap rock of Calton Hill, . . .	121·2
Sand of Experimental Garden, · .	77·19
Sandstone of Craigleith Quarry, . .	273·6

These numbers do not differ much from those given by Professor Forbes, who for the first time derived determinations of thermal conductivity in absolute measure from observations of terrestrial temperature. In consequence of the peculiar mode of reduction. followed in the present investigation, it may be assumed that the estimates of conductivity now given are closer approximations to the truth. To reduce to the English foot as unit of length, we must multiply by the square of 1·06575; to reduce, further, to the quantity of heat required to raise 1 lb of water by 1° as unit of heat, we must multiply by 66·447; and lastly, to reduce to a day as unit of time, we must divide by $365\frac{1}{4}$. We thus find the following results:—

Trap rock of Calton Hill, . . .	23·5
Sand of Experimental Garden, . .	15·0
Sandstone of Craigleith Quarry, . .	53·5

These numbers show the quantities of heat per square foot conducted in a day through a layer of the material one foot thick, kept with its two surfaces at a difference of temperature of one degree,—the unit of heat being, for instance, the quantity required to raise 1000 lbs of water by $\frac{1}{1000}$th of a degree in temperature.

The same system of reduction applied to the observations continued at the Calton Hill station, has led to results from which the following are selected :—

Average annual term for 12 *years*—1842 *to* 1854—*Trap rock to Calton Hill.*

	Col. 1.	Col. 2.	Col. 3.	Col. 4.	Col. 5.
Depths below surface in French feet.	Proportionate Diminution of amplitude.	Diminution of Napierian logarithm of amplitude per French foot of descent.	Retardation of epoch in circular measure, per Fr. foot of descent.	Retardation of epoch in decimal of a year, per Fr. foot of descent.	Retardation of epoch in days, per Fr. foot of descent.
3 feet to 6 feet	·675	·1310	·1233	......	......
6 „ 12 „	·498	·1163	·1142	......	......
12 „ 24 „	·260	·1121	·1145	......	......
3 „ 24 „	·0875	·1160	·1157	·01841	6·724

By these results it will be seen that the discrepancies from the theory based on the hypothetical conditions of uniformity, noticed above as found in the reduction of the first five years' series of observations, are maintained with the same character, and to nearly the same amount, in the succeeding series of thirteen years. An investigation of the changes of conductivity and specific heat, which, if the ground were level and the surface uniform, would be required to account for these discrepancies, is made, so far as the data suffice for determining them. The paper concludes with the solution of some practical problems regarding the conduction of heat through rock possessing the conductivity determined by the reductions which form the chief part of the paper.

4. On a Method of Reducing Observations of Underground Temperatures, with its Application to the Monthly Means recorded in the Report of the Royal Observatory of Edinburgh, &c. By Professor Everett.

In this paper the same general method of reduction as that of Professor Thomson, explained in the preceding paper, is followed. The numerical labour is, however, much diminished by using the monthly means given in the observatory report as data for twelve equations of condition, instead of the methods by which Professor Forbes had obtained data for twelve, and Professor Thomson for thirty-two equations of condition. The method adopted in the present communication, although not susceptible of such minute accuracy as the more elaborate methods referred to, seems to be as accurate as is necessary for a fair representation of the phenomena, and has a great advantage in point of simplicity and ease of working. In the present communication the practical methods of calculating the amplitudes and arguments of the successive terms in the harmonic expression of a periodic variation are fully explained, as it is believed the method will be found useful in the reduction of almost every class of meteorological observations, and as in this country, at all events, there is not much familiarity with it among practical meteorologists.

The following Donations to the Library were announced:—

Herring Fisheries of Scotland. 1860.—*From the Board of Fisheries.*

Comptes Rendus hebdomadaires des Séances de l'Académie des Sciences. No. 16.

Report and Diagram showing the Rise and Fall of the Irawadi River for the Three Years 1856–58.—*From the Secretary of State for India.*

Total Solar Eclipse, 1860, July 18. Revised Path of the Shadow. 8vo.—*From J. R. Hind, Esq.*

The Atlantis. No. V. January, 1860. 8vo.—*From the Catholic University of Ireland.*

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